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ABSTRACTS

SHOULD STRATEGIC STUDIES SURVIVE?

By RICHARD K. BETTS

Political science attends to causes and consequences of war but only fitfully welcomes study of its conduct, because few grasp how much the dynamics of combat shape politics. Bernard Brodie called for development of strategic studies on the model of the discipline of economics, because neither the military nor academia treated the subject rigorously. His call was answered in the early cold war, with mixed results. Theories about nuclear deterrence burgeoned while empirical studies of war lagged. The late–cold war impasse in nuclear strategy, rooted in NATO doctrine, shifted attention to conventional military operations and empirically grounded theory. Since the cold war, research on general theoretical questions about war and peace has been prospering, but education in military matters has been eroding. Interdisciplinary strategic studies integrate political and military elements of international conflict, but there is no recognized discipline of military science; military analysis is smuggled into political science and history departments, where it is resisted by calls to conceptualize security broadly or focus on purely theoretical work. If serious military studies are squeezed out of universities, there will be no qualified civilian analysts to provide independent expertise in policy and budget debates, and decisions on war and peace will be made irresponsibly by uninformed civilians or by the professional military alone.

THEORETICAL DECAY AND THEORETICAL DEVELOPMENT

THE RESURGENCE OF INSTITUTIONAL ANALYSIS

By KAREN L. REMMER

Research on the less industrialized regions of the world has undergone major changes in terms of theoretical rigor, methodological sophistication, and the diversification of analytical approaches since the publication of Samuel P. Huntington's essay, "Political Development and Political Decay," in *World Politics* in 1965. Yet more than three decades later, comparativists are rediscovering political institutions, highlighting the originality of Huntington's scholarly contribution. The resurgence of institutional analysis has redirected attention to the potential variability of political outcomes in the face of sweeping global currents, generated important theoretical insights, and created new bases for dialogue across disparate research traditions. Nevertheless, the horizons of institutional research need to be broadened to address the challenges posed by international influences, two-way interactions between politics and society, and institutional fluidity.

THE ECLIPSE OF THE STATE?

REFLECTIONS ON STATENESS IN AN ERA OF GLOBALIZATION

By PETER EVANS

The economic logic of the current international economy does not predict the "eclipse of the state." Economic globalization does restrict state power, but transnational capital needs capable states as much or more than does domestically oriented business. National success in the current global political economy has been associated not with minimal states but with states that are capable, active, and engaged. Pressure for eclipse flows from the conjunction between transnational economic forces and the political hegemony of an Anglo-American ideology that, in J. P. Nettl's words, "simply leaves no room for any valid notion of the state." Even this combination of economic and political pressure is unlikely to eclipse the state, but it is likely to put public institutions on the defensive, eclipsing any possibility of the "embedded liberalism" described by John Ruggie. A "leaner, meaner" state is the likely outcome. The possibility of a more progressive alternative outcome would depend in part on whether current zero-sum visions of the relation between the state and civil society can be replaced by a more synergistic view.

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LIJPHART, LAKATOS, AND CONSOCIATIONALISM
By IAN S. LUSTICK

Arend Lijphart's 1969 article on consociational democracy was a compelling critique of prevailing theories of democratic stability and the launching pad for one of the most widely regarded research programs in contemporary comparative politics. However, Lijphart and others who adopted consociational approaches encountered severe logical, theoretical, and empirical criticisms of their work. The success of the program and its apparent imperviousness to many of these attacks has been remarkable. Lijphart's primary response was to abandon standard norms of social science in favor of an "impressionistic" approach that protected the attractiveness and wide applicability of the theory at the cost of precision and scholarly rigor. The overall trajectory of the consociationalist research program is explained with reference to a shift from early- to late-Lakatosian commitments—from insisting on corroboration for one's theories through repeated encounters with evidence to a late-Lakatosian stance that expects the political and rhetorical skills of scholars operating on behalf of their research program to be more significant than evidence or theoretical coherence.

PARADIGMS REVISITED
PRODUCTIONISM, GLOBALITY, AND POSTMODERNITY IN COMPARATIVE POLITICS
By ANDREW C. JANOS

Inspired by a seminal essay of Albert O. Hirschman, as well as by the ongoing debate on the empirical foundations of social science, this article "revisits" (1) the paradigm concept popularized by T. S. Kuhn in the 1960s and (2) the relationship between probabilistic and "possibilistic" modes of theorizing that has acquired renewed relevance in comparative politics mainly with respect to recent theories of democratization and development. It does so by reviewing three major paradigm crises in modern political science: the shift from the Aristotelian *polis* to the social "system," the refocusing of political explanations from the social to the global environment, and the contemporary attempts to reevaluate the role of technology in political change. The review takes stock of the record of the discipline of comparative politics, of opportunities provided by paradigm shifts, seized upon or missed by the discipline. It also allows one to seek a more even balance between the potential utility and limitations of the paradigm concept, while at the same time pointing to the perils of divorcing the art of the possible from the laws of probability.

PROBLEMATIC LUCIDITY
STEPHEN KRASNER'S "STATE POWER AND THE STRUCTURE OF INTERNATIONAL TRADE"
By ROBERT O. KEOHANE

Stephen D. Krasner's article in this journal in 1976, "State Power and the Structure of International Trade," defined the agenda for years of scholarship by being both lucid and problematic. Krasner presented a clear puzzle but manifestly failed adequately to answer the questions that he raised. His key proposition, that strong international economic regimes depend on hegemonic power, was supported by only half of the six cases that he discussed. Yet the cogency of Krasner's formulation of the argument, and the pungency of his rhetoric, led "State Power" to serve as a focal point in a coordination game among three major constituencies in the international political economy field. Liberal transnationalists, statist realists, and their audiences all benefited from Krasner's lucid specification of the issues. As a result of research prompted by Krasner's article, we understand the relationship between international political structure and economic openness much better than we did before it appeared.

THE SECURITY DILEMMA REVISITED
By CHARLES L. GLASER

Robert Jervis's article "Cooperation under the Security Dilemma" is among the most important in international relations in the past few decades. Nevertheless, relatively little effort has

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been devoted to examining its core logic, some of which was left incomplete by Jervis. The most important gaps concern whether and how the security dilemma operates between rational actors. The first section of this article closes some of these gaps. The second section argues that two nonstructural variables—the extent of the adversary's greed and the extent of the adversary's unit-level knowledge of the state's motives—influence the magnitude of the security dilemma. The final section addresses basic criticisms of the security dilemma, including the empirical claim that greedy states are the key source of international conflict, that the security dilemma does not really exist, and that offense-defense theory is flawed. I conclude that only the greedy-states criticism poses a serious challenge.

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INTRODUCTION

By HENRY S. BIENEN

MY personal collection of *World Politics* begins with the January 1957 issue: volume 9, number 2. It skips to October 1964 and is complete from then on. When I left Princeton to become Northwestern University's fifteenth president, I gave away most of my professional library, envisioning all too accurately the demands of a new life. I never considered parting with any copies of *World Politics*, however. Perhaps in part this sense of possession came from sentiment arising from my long and happy association with this journal. I think, however, that it stemmed mostly from a recognition that *World Politics* has been the leading journal of international affairs and comparative studies from its founding until now.

I first became aware of the journal's importance as an undergraduate government major at Cornell in the late 1950s. Later, as a graduate student in political science at the University of Chicago in the early 1960s, I was struck by how often my professors assigned articles from *World Politics*. Sad to say, I was also struck to find that some of those articles had been cut out of the issue on library reserve. The vandalism was a testament—albeit an unfortunate one—to the importance and centrality of the articles published in *World Politics*.

As I look back over the almost thirty years that I was associated with *World Politics*—an association that progressed from my status as reader, to associate editor, review articles editor, editorial committee member, and chair of the editorial committee—I see both continuity and change. I hope that I will be allowed some personal reflections about these continuities and changes.

I became an assistant professor at Princeton University in 1966. Klaus Knorr was director of the Center of International Studies at the time, as well as chair of the editorial committee of *World Politics*. Klaus invited me to join the center as a faculty associate and to become a reader for *World Politics*. I was very pleased: in those days, and to some extent even now, a Princeton professor (usually of politics) served a probationary period as a reader for the journal; those who did their job well graduated to associate editor. Doing one's job well meant reading

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quickly, as well as diligently and intelligently: this was a journal that prided itself on getting a decision to authors quickly. *World Politics* also turned to readers outside of Princeton and continues to do so.

Over the course of fifty years, *World Politics* was characterized by both continuity and change in terms of its personnel and editorial and administrative structure. In all these years there have been only six chairpersons of the editorial board: Frederick S. Dunn, Klaus Knorr, Cyril Black, myself, John Waterbury, and Michael W. Doyle, the present chair. The board expanded over time, and so did the number of working editors. Whereas in 1948, when the journal was still at Yale's Institute of International Studies, there was a small editorial board and a group of advisory editors, today the academic needs of the journal are served by three editorial bodies: the editorial board, which operates in an advisory capacity, consists of scholars at other institutions; the editorial committee is the core group, the decision-making body, and is drawn from the Princeton faculty; and a group of associate editors, also from Princeton, referees articles on a regular basis. In addition, readers from other institutions—as many as fifty in a given year—are called on to referee articles for the journal. On the administrative end, an executive editor, a nonacademic, eventually replaced the managing editor, a role originally filled by an academic. *World Politics* has also been somewhat unusual among academic journals in having had for most of its history a full-time professional editor. There has also been remarkable continuity in that the position, by whichever of its names, has been filled by only five people in fifty years. (Indeed, Elsbeth Lewin held the potition for twenty years.) As the tasks performed by the executive editor expanded, there was need to add to the staff, and since the late 1980s *World Politics* has also had an assistant editor.

Over the years *World Politics* has consistently ranked among the very best journals of both international affairs and comparative politics. The majority of the authors published in the journal have been Americans, but there has always been some representation from abroad as well. Most of the authors have been political scientists—area specialists, political historians, economists turned political scientists, those who used more quantitative analysis and those who did mathematical modeling. But others have also frequented the journal's pages as well: historians, sociologists, anthropologists, psychologists, international lawyers, and economists. Over the years the journal has had mostly male authors and editors. But the number of female writers and editors has been growing. Indeed, by happenstance, all of the authors contributing to the April 1993 issue were women.

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What is striking is how eclectic a journal *World Politics* has remained. Some international affairs journals stress policy analysis; others specialize in a particular area of the world; yet others feature comparative studies more than international relations. *World Politics*, by contrast, was among the very best quarterlies in all the areas in which it published and it remains so. Ask Africanists, East Asian specialists, Latin Americanists, Soviet and then Russian experts, European analysts: which journals of international and comparative studies do you read? The list for most will include *World Politics.* For area specialists, the journal of choice for publishing their work is often *World Politics,* rather than a regionally based journal. Moreover, it is the rare government agency that deals with foreign policy or an area of the world that does not have *World Politics* in its library, even though the journal is an academic enterprise, not per se a policy journal and not aimed at a lay audience. And any anthology that included major articles in comparative politics, diplomatic history, American foreign policy, security studies, international political economy and its subareas of trade, economic assistance, and monetary policy would have a disproportionate representation of articles from *World Politics.* Similarly, any anthology organized around theoretical analysis of modernization and political development, democracy and dictatorship, political leadership, ethnicity and communal conflict, revolution, and rebellion, the evolution of states and individual country or area studies of these subjects featured articles from *World Politics.*

World Politics has always been known for publishing innovative work in strategic and security studies and alliance systems and new approaches to conflict and cooperation, arms control, and arms races. Indeed, what was new and innovative was sometimes the application of a heretofore neglected discipline to a field of study in international affairs, for example, psychological approaches to security studies. Or it might be the development of game-theoretic techniques for understanding cooperation and conflict or the application of microeconomics and rational choice theories to studies of coalitions or rebellion and revolution.

The first decade of *World Politics* had more of a focus on international relations than on comparative politics or single countries; within international studies there was more diplomatic and economic history published then than from the 1960s on, when strategic studies came to take up more pages in the journal. And from the 1960s into the 1970s new nations, modernization, and development came to preoccupy the authors of *World Politics.* The journal reflected a world that had changed since the end of the Second World War. These changes—the

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existence of new states, the perception of the salience of a communist bloc, the threat of nuclear arms—were all subjects to be addressed analytically. These topics were prevalent in the 1980s and remain so in the 1990s. There is, perhaps, more attention paid today to explicitly normative issues; a concern with the breakdown of states and regimes; a questioning of the ability to construct viable international institutions; and a somewhat greater focus on international political economy as compared with security issues.

Many of the most interesting articles throughout the life of the journal were its review articles. I served as review articles editor for a number of years and enjoyed that job immensely. *World Politics* may not have been the first to use the review essay format, but it was certainly among the most successful. *World Politics* review articles were often among the most cited articles in the journal. What was so striking about a first-rate review article in *World Politics*? It could define a field or an approach or the analysis of a country or region. The authors always picked a problem in analysis and used the books under review to reflect on the problem topic, both summing up a subject and pointing the way to fruitful modes of analysis for future investigation. The idea was not to review books so much as to define the contours of interesting subjects and problems and to cut through to key issues in analysis. Review articles have been among the classics published in *World Politics*, and among the first published articles of many a fine scholar was a *World Politics* review article. Perhaps a collection of them would be a fit subject for the hundredth-year anniversary issue!

In fact, *World Politics* has published rather few special issues.[1] But for this milestone fiftieth anniversary, the editors decided to commission essays for a special issue by some of today's outstanding scholars. Each of these individuals was invited to revisit a seminal article published in *World Politics*, an article that influenced the development of a field. To that end, seven articles were selected, drawn from the first thirty years of publication by *World Politics*. These are articles that have had time to accumulate a history. The number is of necessity modest, given the constraints of space imposed by a single issue of the journal; obviously, seven essays—whether old or new—cannot do justice to the variety, complexity, and perhaps pioneering quality of *World Politics*. But it is the hope of the editors that readers will consider these articles to be a

[1] *World Politics* has published the following special issues: vol. 14 (October 1961), entitled "The International System: Theoretical Essays"; a supplement to vol. 24 (1972), entitled "Theory and Policy in International Relations"; vol. 38 (October 1985), entitled "Cooperation under Anarchy"; vol. 41 (January 1989), entitled "The Rational Deterrence Debate: A Symposium"; vol. 44 (October 1991), entitled "Liberalization and Democratization in the Soviet Union and Eastern Europe."

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worthy choice for reconsideration and the new essays to be a contribution to the thinking in the various fields.

In a way, the essays in this issue follow in the tradition of the fine *World Politics* review article. In this case, the featured item is of course a single article, but the reviewer examines the earlier author's views in the light of the work of many others as well. The focus is on an issue-area and an exploration of the development of analytical issues as set out earlier on.

A new generation of scholars may read into authors' work what is not there or, more usually, may emphasize different aspects of that work than the original authors did. Perhaps this is to be expected. Thus, it should not surprise us that the authors looking back on earlier articles see those works through the prism of work done subsequently and within the context of their own set of scholarly concerns. This is not to say that the authors whose earlier work is probed here should not be subject to contemporary critique. That is, after all, the point of the special issue.

One will find in this special anniversary issue the following essays.

Richard K. Betts looks at the work of Bernard Brodie and departs from Brodie's position. He poses the question, what is the field of strategic studies, and what should it be? And he examines elements of power and analyzes it as a critical component of policy-making, while commenting on Knorr's work, too, as well as on work done at Rand and elsewhere.

Karen L. Remmer reflects on the earlier contribution of Samuel P. Huntington. Seeing his work has pioneering, she also challenges some of his formulations for being "less than precise" and for not providing a testable theory.

Peter Evans reexamines J. P. Nettl's ideas about the state and "stateness," by, among other things, looking at the evolution of the international system and at the changes wrought by international economic transactions. Evans explores ways that we must reconceptualize the state while examining its relations to civil societies.

Ian S. Lustick reassesses the work of Arend Lijphart on consociational democracy in true review article mode. Citing a vast literature, he evaluates Lijphart's work in terms of evaluative categories defined by Imre Lakatos.

Andrew C. Janos focuses on the idea of paradigm, not surprisingly, since Albert O. Hirschman took up the subject in a classic article. True to Hirschman, Janos also explores the relationship of theory to the practice of leadership and innovation and shares Hirschman's concern that social theory be ethical.

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Robert O. Keohane, considering the work of Stephen D. Krasner, explores how we reach conditional generalizations as we try to draw boundaries around a subject, in this case, international political economy. He examines why Krasner's arguments have been so influential. They partially stand up and partially do not, but this, says Keohane, is how we push knowledge forward.

Charles L. Glaser accepts the importance of Robert Jervis's work and examines why the paradigm of realism became dominant. He explores criticism of Jervis's work to see how his idea of the security dilemma holds up and, belatedly, how examining theories of offense-defense and greed versus security might make us think about cooperation and conflict.

I will close with the following reflection on this endeavor: that what we ask of great articles is not that they answer questions definitively in broad fields such as comparative development, international security, and international political economy but that they pose questions in fruitful ways, point out directions in which to take our future empirical work, and provide frameworks, concepts, and, one hopes, testable theory. In this way we celebrate old work even as we explore new paths.

Not all articles published during the first fifty years of *World Politics* meet those tests. Still, even work that fails the test can be interesting and even great. *World Politics* has published more than its share of such work. Has any other journal of international affairs done better? I do not think so.

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SHOULD STRATEGIC STUDIES SURVIVE?

By RICHARD K. BETTS*

A specter is haunting strategic studies—the specter of peace. This sounds odd so long after the burst of euphoria at the end of the cold war, which dissipated into so many nasty little wars. Political science, however, has been less interested in war per se than in cataclysmic war among great powers, war that can visit not just benighted people far away, but people like us. Half a century of world war and cold war provided that impetus for strategic studies. After the cold war, however, universities face other demands as resources shrink. Has the warrant for feeding this field expired? Certainly not.

First, one interest alone fully justifies keeping the flame burning: to have expertise on the shelf in case great-power conflict arises again, which is more likely to happen than not. For whatever reason, the United States finds itself in a war or crisis in almost every generation.

Second, confusion continues about what U.S. foreign policy should expect military power to do for less vital interests. What force can accomplish in a specific situation does not follow directly from standard international relations theories or rational choice models; the answer depends on military technology, organization, and doctrine, and how they fit with local political and geographic circumstances. After the cold war, liberals, on the one hand, who spent the last thirty years trying to reduce American military power, demanded that Washington "do something" with the armed forces to suppress atrocities, promote democracy, and keep peace in places like Bosnia, Somalia, and Haiti. Conservatives, on the other hand, insisted on buying hefty forces but not using them. Vague notions that military power can impose political solutions at a reasonable cost, or that outside military power is useless for doing so, were subjected to little analytical discipline after 1990. If capacity for informed strategic analysis—integrating political, eco-

* Thanks to Robert Art, David Baldwin, Michael Desch, Peter Feaver, Stephan Haggard, Michael Handel, Samuel Huntington, Robert Jervis, Miles Kahler, David Lake, Michael Mandelbaum, John Mearsheimer, Barry Posen, Cynthia Roberts, Gideon Rose, John Ruggie, Warner Schilling, Jack Snyder, Barry Steiner, Marc Trachtenberg, and Stephen Walt. The value of their criticisms exceeded my ability to incorporate them within length restrictions, which also limited bibliographical footnotes to illustrative examples rather than recognition of the full range of important works.

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nomic, and military judgment—is not preserved and applied, decisions on the use of force will be uninformed and, therefore, irresponsible.

Third, the size and composition of the U.S. defense budget are crucial, affecting fiscal and social policy as well as foreign affairs. Who can rationally recommend whether the budget should be higher or lower, or what it should buy, without any expertise on the nature of military forces and what combinations of them are necessary to achieve objectives set by elected officials? If civilian strategists are not to decide along with the professional military, either ignorant civilians will do it, disjoining political and military logic, or the military will do it alone.

Fourth, U.S. civil-military relations are problematic. The armed forces were reformed and rejuvenated over the same time that political leadership loosened oversight. Reagan's romantic nationalism made for laissez-faire civilian control, and Clinton's impaired moral authority, owing to his own draft evasion, precluded vigorous guidance as commander in chief. After Vietnam, the military became more popular with the mass public as the elite distanced itself from it. Fewer civilian policymakers have experienced military service themselves, while the military institution as it shrinks is growing apart from society after a half century of closeness enforced by the mass mobilization of world war and cold war. There is no danger of direct insubordination, but a larger proportion of military officers now feels more competent and more moral than the rest of their country and less respectful of their government. Education in strategy will not solve problems in civil-military relations and might even aggravate conflict if it emboldens civilians to question military judgments. But if checks and balances matter, it can only help.

Strategic studies is both necessary and contested because it focuses on the essential Clausewitzian problem: how to make force a rational instrument of policy rather than mindless murder—how to integrate politics and war. This requires the interdisciplinary joining of military grammar and political logic, in Clausewitz's terms, a marriage that gets lip service in principle but is often subverted in practice by those who identify more with one half of the union than the other. Soldiers often object to politics permeating war because it gives civilians the right to meddle in operations, while many intellectuals object to dignifying war as an instrument of policy or an academic priority. For all these reasons, political science became the main academic home for the field, and the place of military affairs within it is periodically challenged.

Within a field of international relations constantly riven by sectarian debates about overarching frameworks like realism, liberalism, and their

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"neo" variants, the murky boundaries of strategy fuel controversy. To clarify where strategic studies *should* fit, think of a subfield of three concentric circles: at the core is *military science* (how technology, organization, and tactics combine to win battles); the outer, most inclusive ring is *security* studies (everything that bears on the safety of a polity); and in the *middle* lies *strategic* studies (how political ends and military means interact under social, economic, and other constraints).

The distinctions are relevant in principle, because they illustrate why strategic studies should be the most important part of the subfield—broader in scope than strictly military problems, but more focused than security studies, which is potentially boundless. In practice, however, the distinctions solve few problems because the dividing lines between strategic studies and the other two layers can never be clear, and the distinctions are not recognized institutionally. Only security studies has academic standing, so the place of strategic studies emerges through debates about defining security. Most scholars of security identify it with strategic studies, but much of what they do strikes some in other subfields as too close to military science for comfort. Critics then argue for reorienting the security subfield to so many other issues that the military core may become a pea lost in an amorphous ball of wax. The intellectual coherence of strategic studies increases with linkage to the military core, but institutional status and legitimacy grow with distance from it.

One danger in strategic studies is missing the political forest for the military trees. That danger was greater during the cold war than now. The opposite danger—that defining security broadly will squeeze out work on the military aspects—is greater now. There is no consensus that attention to military matters remains an important responsibility for social science, or even that knowledge of military systems is as vital for studying security as knowledge of economic systems is for studying political economy.

The Case for Scientific Strategy

The case for strategic studies had to be made a half century ago as well. Bernard Brodie's 1949 article, "Strategy as Science," was a brief for developing strategy as a systematic field of analysis because it was "not receiving the scientific treatment it deserve[d] either in the armed services or, certainly, outside of them."[1] The only scholars who had paid much attention to the subject up to that point were historians. The

[1] Brodie, "Strategy as a Science," *World Politics* 1 (July 1949), 468.

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methodological model that Brodie endorsed was the one represented by the discipline of economics.

Perhaps Brodie should have heeded the warning to be careful what you wish for, lest you get it. Much of what he recommended came to pass, but with results that did not entirely please him or critics who had little use for his aim from the beginning. Brodie had in mind an instrumental science for solving practical problems. This evoked skepticism on two fronts. Although the services sometimes welcomed analysis by civilian scholars, many military professionals regarded outsiders' work on strategy as impertinent interference. Although scholars of strategy established lodgments in universities and think tanks, many intellectuals saw them as unprofessional or immoral, considering instrumental science inferior to loftier theoretical work, or, when applied to managing violence, the work of the devil.

Most scholars of international relations recognize that war is an important problem but are interested only in the before and after, not in war itself—in war's causes and consequences, but not its conduct, which is considered somehow epiphenomenal or intellectually puerile. Strategic studies is concerned with all three phases of war because they are interdependent; conduct becomes cause, as mechanisms of violence shape decisions about its political application. It is impossible to understand impulses and choices in the political dimension of war or peace without understanding constraints and opportunities in the military dimension. Options for *how* to make war affect *whether* war is made, who wins or would win, and thereby the shape of the postwar world (or the peacetime world, if anticipated results of combat affect diplomatic deals). For example, it is not possible to understand how Germany managed to rule Europe for half of the 1940s without understanding how it overcame the opposing might of France and Britain as it had not been able to do in 1914. This cannot be explained by indices of power (GNP, population, the size of armed forces) that are accessible to nonspecialists but only by grasping innovations in the process of combat—how the Wehrmacht adapted the technology and doctrine of armored warfare to revolutionize operations. Similarly, one cannot understand why Germany ultimately failed by looking at military science, but only by looking to wider dimensions of strategy—the ideological and psychological reasons for Hitler's miscalculations in invading the Soviet Union and declaring war on the United States.

Intellectual support for strategic studies parallels cycles of international conflict and calm. When the danger of war obtrudes in the real world, the study of war prospers, because the academy considers it un-

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avoidable. When danger slackens, academic interest or tolerance falter. Two decades after "Strategy as a Science" was published, as Vietnam was destroying the cold war consensus, Hedley Bull noted that the professional strategist's status was tenuous due to controversy over the legitimacy of the very question at issue: "What shall the state do with its military force? . . . [T]here will not be general agreement about the worth and utility of students of strategy, in the way that there is . . . about that of students of medicine, architecture, or economics."[2] Nearly half a century after Brodie's article, in the happy wake of the cold war, David Baldwin argued that "perhaps the time has come to abolish the subfield of security studies."[3]

The intellectual advances Brodie sought in 1949 did not solve all the problems he saw, and created some new ones. In the enthusiasm for science, strategic studies developed a scientistic strain and overreached. Nevertheless, with later leavening of the scientism by better comparative historical analysis in the second half of the cold war, Brodie's brief yielded progress. If Baldwin's advice prevails, the problems that motivated Brodie—the superficial quality of analysis available to support public decisions about war and peace, and the absence of civilian analytical checks on preferences of the professional military—will grow again.

Brodie spoke as the Clemenceau of the academy: strategy was too important to be left to the generals. As one who knew military history and moved among those in uniform as a wartime officer and peacetime consultant, he was frankly cynical about the cultural and organizational constraints that inhibited serious strategic analysis by soldiers themselves. He considered professional officers unattuned to strategy because the complexity of military operations made them preoccupied with tactics and technology. He believed that regular officers view strategy in terms of the hallowed "Principles of War" (maxims about "the objective," "economy of force," "unity of command," and so forth that appear in manuals of most Western armies), that they have difficulty grasping the real meaning of Clausewitz's insight on the relation between war and politics, and that anti-intellectualism and hierarchy prevent trenchant thought. In Brodie's view, "political scientists . . . are concerned with the *context* of military operations," whereas "to the military, the means available, rather than the object, are what determine the character of a war" (pp. 467–68, 473, 486).[4]

[2] Hedley Bull, "Strategic Studies and Its Critics," *World Politics* 20 (July 1968), 596.

[3] David Baldwin, "Security Studies and the End of the Cold War," *World Politics* 48 (October 1995), 135.

[4] See also Bernard Brodie, *Strategy in the Missile Age* (Princeton: Princeton University Press, 1965), 11, 13; idem, *War and Politics* (New York: Macmillan, 1973), 9–11; idem, "Scientific Progress and Political Science," *Scientific Monthly* 85 (December 1957), 317.

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Since military authors are tied to their services, it is hard for anyone but a civilian to proffer analysis independent of service doctrine. (There are exceptions. Perry Smith published an unflattering account of his service's strategic planning, yet survived, through the support of a patron, and reached two-star rank himself. Andrew Krepinevich savaged his service's doctrine in the Vietnam War, but finished his career as a lieutenant colonel working in the civilian reaches of the Pentagon.[5] Most officers who challenge their services wait until retirement.) Moreover, the nuclear revolution put the dominant level of warfare beyond experience, which is the main teacher in the military ethos. Thus when strategic studies burgeoned in the 1950s, most of the writing was by civilians.

As Brodie noted in 1949, "The military profession is by no means alone in its frequent recourse to the slogan as a substitute for analysis—certain scholarly disciplines, not excluding political science, have been more than a little untidy in this regard" (p. 471). He saw economics, the most developed social science, as the model because strategy is about "problems involving economy of means, *i.e.,* the most efficient utilization of potential and available resources" (p. 475). Choices in weapon procurement, for example, should not be governed by slogan-like concepts like "balanced force," but by marginal utility (pp. 478–81).

All of this anticipated currents that would dominate the development of strategic studies in the first half of the cold war. Brodie wrote his article while at Yale, but at the same time that he was beginning his affiliation with the fledgling RAND Corporation. Established by the Air Force, RAND became a magnet for those who wrestled intellectually with the strategic challenge of the nuclear revolution. Some like Brodie, William Kaufmann, and Alexander George were political scientists versed in history, but most were mathematicians, physicists, or economists like Albert Wohlstetter, Herman Kahn, Thomas Schelling, James Schlesinger, Andrew Marshall, Henry Rowen, Malcolm Hoag, Carl Kaysen, and Daniel Ellsberg. This group spawned much of the theoretical corpus that undergirded academic study of strategy during the cold war.[6]

[5] Smith, *The Air Force Plans for Peace, 1943–1945* (Baltimore: Johns Hopkins Press, 1970); Krepinevich, *The Army and Vietnam* (Baltimore: Johns Hopkins University Press, 1986).

[6] Fred Kaplan, *The Wizards of Armageddon* (New York: Simon and Schuster, 1983); Lawrence Freedman, *The Evolution of Nuclear Strategy*, 2d ed. (New York: St. Martin's Press, 1989); Barry Steiner, *Bernard Brodie and the Foundations of American Nuclear Strategy* (Lawrence: University Press of Kansas, 1991).

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The First Cycle of Cold War Strategic Studies

The year after Brodie's article appeared the Korean War confirmed the militarization of the East-West conflict, U.S. defense spending tripled, NATO became the centerpiece of foreign policy, and strategy became big business. In universities, realist theory and security policy took over the field of international relations, eclipsing the subfields of international law and organization that had dominated in the interwar years. In the 1950s and 1960s the Social Science Research Council's (SSRC) Committee on National Security Research under William T. R. Fox built a network of academics. University programs sprang up at: Princeton's Center of International Studies, where Klaus Knorr theorized about war potential, economic mobilization, and NATO strategy, and which produced works on deterrence by Glenn Snyder, William Kaufmann, and Herman Kahn; Columbia's Institute of War and Peace Studies, which sponsored research by Kenneth Waltz, Samuel Huntington, Paul Hammond, Warner Schilling, and others on causes of war and defense policy-making; Ohio State's Mershon Center, which supported not only mainstream research on security, but critics as well, such as Philip Green; Harvard's Center for International Affairs, where Henry Kissinger continued to make his mark after the publication of his Woodrow Wilson Award–winning book on nuclear strategy for the Council on Foreign Relations; and MIT's Center of International Studies (and later its Defense and Arms Control Studies Program). In London, the International Institute for Strategic Studies was established and has since provided a steady stream of analytical publications and unclassified data compilations.[7]

Professors jumped into policy prescription, beginning with *The Absolute Weapon*, edited by Brodie.[8] Strategy might not have developed academically outside of military history if not for the nuclear revolu-

[7] Knorr, *The War Potential of Nations* (Princeton: Princeton University Press, 1956); Knorr, ed., *NATO and American Security* (Princeton: Princeton University Press, 1959); Snyder, *Deterrence and Defense* (Princeton: Princeton University Press, 1961); Kaufmann, ed., *Military Policy and National Security* (Princeton: Princeton University Press, 1956); Kahn, *On Thermonuclear War* (Princeton: Princeton University Press, 1960); Waltz, *Man, the State, and War* (New York: Columbia University Press, 1959); Huntington, *The Common Defense* (New York: Columbia University Press, 1962); Schilling, Hammond, and Snyder, *Strategy, Politics, and Defense Budgets* (New York: Columbia University Press, 1962); Demetrios Caraley, *The Politics of Military Unification* (New York: Columbia University Press, 1966); Michael Armacost, *The Politics of Weapons Innovation* (New York: Columbia University Press, 1969); Green, *Deadly Logic* (Columbus: Ohio State University Press, 1966); Kissinger, *Nuclear Weapons and Foreign Policy* (New York: Harper, 1957). IISS publications include the journal *Survival*, the Adelphi Papers, and the annuals *Military Balance* and *Strategic Survey*.

[8] Brodie, ed., *The Absolute Weapon* (New York: Harcourt, Brace, 1946).

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tion. Nuclear war spurred theorizing because it was inherently more theoretical than empirical: none had ever occurred. Except for Hiroshima and Nagasaki, where there was no question of retaliation, there was no messy store of historical evidence to complicate elegant abstractions. Available empirical data were technical—the physics of fission, fusion, and ballistics—and the implications appeared simple: for the first time, great powers would have the option to annihilate enemy societies overnight. Since no one had experience, intellectuals felt less inhibited by military expertise. Alain Enthoven, the prototypical Pentagon "whiz kid," was notorious for his arrogant comment in a dispute over strategic plans: "General, I have fought just as many nuclear wars as you have."[9]

With scant empirical grounds for testing propositions, nuclear strategy and deterrence seemed perfectly suited to deductive logic and game theory. A few simple ideas, based on a small number of assumptions and variables, seemed extremely powerful. By the 1960s theorists had highly developed ideas about how to organize nuclear capabilities to stabilize U.S.-Soviet deterrence. Arguments among strategists from the ivory tower about logical effects of "invulnerable second strike capability," "reciprocal fear of surprise attack," "counterforce options," "mutual assured destruction," "graduated escalation," and "crisis stability" had a profound influence on civilian leaders.[10]

As long as nuclear weapons remained leashed and strategy seemed successful, strategic studies prospered. At the opposite end of the spectrum from nuclear war, however, strategy did not prosper. After the Cuban missile crisis, the focus of East-West competition shifted to the Third World. Many strategists turned their attention to problems of counterinsurgency. In this realm, in contrast to nuclear abstraction, theories *were* mercilessly subjected to testing. Most nonspecialists saw Vietnam (rather than successful cases of counterinsurgency in Greece, Malaya, and the Philippines) as the test and as evidence that theories failed when applied. Most of the work on counterinsurgency by professional analysts, however, was case-study research, and most of the theories came from practitioners.[11] Theoretical breakthroughs in the first

[9] Quoted in Kaplan (fn. 6), 254.

[10] Marc Trachtenberg, *History and Strategy* (Princeton: Princeton University Press, 1991), chap. 1; Patrick Morgan, *Deterrence*, 2d ed. (Beverly Hills, Calif.: Sage, 1983); Richard Betts, "Nuclear Weapons," in Joseph Nye, ed., *The Making of America's Soviet Policy* (New Haven: Yale University Press, 1984); Robert Jervis, *The Illogic of American Nuclear Strategy* (Ithaca, N.Y.: Cornell University Press, 1984); Colin Gray, *Nuclear Strategy and National Style* (Lanham, Md.: Hamilton Press, 1986); Lynn Eden and Steven Miller, eds., *Nuclear Arguments* (Ithaca, N.Y.: Cornell University Press, 1989).

[11] *Selected Works of Mao Tse-tung*, vol. 2 (Peking: Foreign Languages Press, 1965); Jay Mallin, ed., *"Che" Guevara on Revolution* (Coral Gables, Fla.: University of Miami Press, 1969). Academics

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cycle of strategic studies had been more about deterrence, nuclear strategy, and escalation than about revolution, intervention, and subconventional war. Apart from whatever credit it might claim for helping to prevent World War III, the field's weakness in the first cycle was the overwhelming attention given to the least likely type of war and the late consideration of the most likely. Given the utilitarian rationales for the field, it is hardly surprising that critics saw the Vietnam disaster as a reflection on it.

The other area in which analysts became influential in policy was defense program management. RAND provided not only deterrence theorists but cost-effectiveness experts to McNamara's Pentagon. Along with the unprecedented supervision of military operations in the air war over Vietnam, the managerial revolution was a prime precipitant of civil-military friction. To some, the military reaction to the civilian analysts evinced the anti-intellectualism that Brodie complained about in 1949, with military vested interests resisting dispossession as new players sought to rationalize the allocation of marginal resources.[12] In other respects, proponents of cost-effectiveness criteria overplayed their hand, blithely overruled traditional military judgment, and revealed the limits of economic analysis as a basis for military decision.[13]

In the 1960s Brodie made a midcourse correction. He rethought his enthusiasm for economic conceptualization of strategy, worrying that the approaches he had recommended in 1949 had been taken much

developed limited war theories mostly about Korea and NATO, not subconventional war. Robert Osgood, *Limited War* (Chicago: University of Chicago Press, 1957); Henry Kissinger, *The Necessity for Choice* (New York: Harper, 1961); Morton Halperin, *Limited War in the Nuclear Age* (New York: Wiley, 1963). On unconventional war in the third World, French and British colonial veterans wrote theoretical statements: David Galula, *Counterinsurgency Warfare* (New York: Praeger, 1964); Robert Thompson, *Defeating Communist Insurgency* (New York: Praeger, 1966). One of the few theoretical works by academics that holds up is Samuel Huntington, "Patterns of Violence in World Politics," in Huntington, ed., *Changing Patterns of Military Politics* (New York: Free Press of Glencoe, 1962). Examples of case studies include Lucian Pye, *Guerrilla Communism in Malaya* (Princeton: Princeton University Press, 1956); Douglas Pike, *Viet Cong* (Cambridge: MIT Press, 1966); Jeffrey Race, *War Comes to Long An* (Berkeley: University of California Press, 1972). For postmortems, see Douglas Blaufarb, *The Counterinsurgency Era* (New York: Free Press, 1977); Larry Cable, *Conflict of Myths: The Development of American Counterinsurgency Doctrine and the Vietnam War* (New York: New York University Press, 1986); D. Michael Shafer, *Deadly Paradigms* (Princeton: Princeton University Press, 1988); Timothy Lomperis, *From People's War to People's Rule* (Chapel Hill: University of North Carolina Press, 1996).

[12] The seminal cost-effectiveness work on defense management is Charles Hitch et al., *The Economics of Defense in the Nuclear Age* (Cambridge: Harvard University Press, 1960). See also E. S. Quade and W. I. Boucher, eds., *Systems Analysis and Policy Planning Applications in Defense* (New York: American Elsevier, 1968).

[13] Robert Art, *The TFX Decision* (Boston: Little, Brown, 1968); Robert Coulam, *Illusions of Choice* (Princeton: Princeton University Press, 1977). McNamara's apostles claimed not to claim too much. See Charles Hitch, *Decision-Making for Defense* (Berkeley: University of California Press, 1965), 76; but as correctives, J. A. Stockfisch, *Plowshares into Swords* (New York: Mason and Lipscomb, 1973), 197; and James Schlesinger, "Uses and Abuses of Analysis," in U.S. Senate, Committee on Government Operations, *Planning Programming Budgeting* (Washington, D.C.: GPO, 1970).

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farther than he had expected, and that tools that were useful for limited purposes had been abused to answer questions beyond their applicability. Leaving RAND for UCLA in 1966, he was appalled by the "astonishing lack of political sense" and the ignorance of diplomatic and military history that he saw among economists who had become eminent strategists. "It is not that they have no time for history but rather that the devotees of any highly developed science . . . tend to develop a certain disdain and even arrogance concerning other fields." In 1949 he had seen professional soldiers as too limited by soft intuition and folklore; in the 1960s he believed economics could do no better without incorporating more of the knowledge that scientists often consider soft.[14] By the 1970s, however, he need not have worried. Having played a central role in development of deterrence theory, economists were by then found hardly anywhere in the academic study of military affairs. RAND had also evolved into a bureaucratized contract research organization as much as a think tank and was no longer the hothouse of theoretical ferment it had been in the 1950s.

For a time no one took up the slack. Vietnam poisoned the academic well, and détente removed the urgency about deterrence. For a decade after the late 1960s, little serious work on military affairs was undertaken in universities, apart from arms control studies. The 1970s produced ample work on U.S.-Soviet negotiations, much of it a valuable extension of ideas developed earlier,[15] but most of which was technical and ahistorical. The Ford Foundation established research centers that concentrated on arms control at Harvard, Stanford, MIT, and Cornell. Systems analytic techniques were applied to defense program issues in monographs put out by the Brookings Institution, which influenced Washington policy debates of the 1970s but were not designed to advance theoretical debates (subsequent Brookings studies moved in that direction).[16] Later, the MacArthur Foundation dispensed numerous grants but emphasized nonmilitary subjects.

[14] Trachtenberg (fn.10), 13n; Brodie, quoted in Steiner (fn. 6), 196–97; Brodie, "Why Were We So (Strategically) Wrong?" *Foreign Policy,* no. 5 (Winter 1971–72), 154. One of the principals who imposed economic analysis in the Pentagon foresaw the problem; Charles Hitch, "National Security Policy as a Field for Economics Research," *World Politics* 12 (April 1960), 448.

[15] Donald Brennan, ed., *Daedalus* 89, special issue (Fall 1960); Thomas Schelling and Morton Halperin, with the assistance of Donald Brennan, *Strategy and Arms Control* (New York: Twentieth Century Fund, 1961).

[16] Examples of Studies in Defense Policy published by the Brookings Institution in Washington, D.C., in the trough between the first and second cycles include Martin Binkin, *Support Costs in the Defense Budget* (1972); William White, *U.S. Tactical Airpower* (1974); Barry Blechman, *The Control of Naval Armaments* (1975). More academic Brookings publications in the second cycle include Joshua Epstein, *The Calculus of Conventional War* (1985); Richard Betts, *Nuclear Blackmail and Nuclear Balance* (1987); Thomas McNaugher, *New Weapons, Old Politics* (1989); Bruce Blair, *The Logic of Accidental Nuclear War* (1993).

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There was also a counteroffensive against the dominance of strategic studies over the field of international relations. Scholars advocated shifting the focus to interdependence and political economy because the importance of states and the utility of force had declined.[17] By the end of the 1970s, however, the tide turned. Klaus Knorr—an early colleague of Brodie, editor of *World Politics*, one of the few economists who kept working on questions of national security after the 1960s, and one of the few strategists to integrate political, military, and economic analysis—had been among the first to argue the declining utility of force. Like Brodie, however, Knorr became alarmed by those who took his argument too far and within a decade was publishing reconsiderations.[18] Within a few years of publishing *Power and Interdependence*, moreover, Joseph Nye turned his own interests toward security issues.

The Second Cycle and After

The hiatus in strategic studies ended with the revival of the cold war at the close of the Carter administration. The logistical base for the field grew. In the first cycle, *World Politics* was the main outlet for academic articles on strategy. In the second cycle, specialized journals came to the fore, especially *International Security*.[19] In the first cycle, ideas revolved around basic concepts (deterrence, stability, credibility). In the second cycle, debate was about the elaboration of concepts, variations on old themes, and how specific configurations of capability would buttress or undermine peace.[20] In the second cycle, the most novel research and theoretical development took an empirical turn.

One area that opened up at the end of the 1970s was strategic intelligence. A few excellent works on the subject had appeared early in the cold war because political pressure to account for disasters eased re-

[17] Robert Keohane and Joseph Nye, *Power and Interdependence* (Boston: Little, Brown, 1977), chap. 2.

[18] Knorr, *On the Uses of Military Power in the Nuclear Age* (Princeton: Princeton University Press, 1966); idem, "On the International Uses of Military Force in the Contemporary World," *Orbis* 20 (Spring 1977); Richard Betts, Michael Doyle, and John Ikenberry, "An Intellectual Remembrance of Klaus Knorr," in Henry Bienen, ed., *Power, Economics, and Security* (Boulder, Colo.: Westview Press, 1992), 17–19.

[19] See also *The Journal of Strategic Studies*, *Survival*, *Defense Analysis*, *Comparative Strategy*, *Arms Control*, and *Small Wars and Insurgencies*. One of the better journals, *Security Studies*, began publishing after the cold war ended. Official journals include *Naval War College Review*, *Parameters*, and *Joint Force Quarterly*.

[20] Creative revisitations included Michael Mandelbaum, *The Nuclear Revolution* (New York: Cambridge University Press, 1981); Richard Ned Lebow, *Nuclear Crisis Management* (Ithaca, N.Y., Cornell University Press, 1987); Robert Jervis, *The Meaning of the Nuclear Revolution* (Ithaca, N.Y.: Cornell University Press, 1989). See also Michael Howard, "The Forgotten Dimensions of Strategy," *Foreign Affairs* 57 (Summer 1979).

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strictions on information about a few cases. Roberta Wohlstetter's classic book, *Pearl Harbor*, was based on thirty-nine volumes of congressional hearings, and Klaus Knorr's article, "Failures in National Intelligence Estimates," drew on his involvement in the postmortem of the Cuban missile crisis by the intelligence community's Board of National Estimates.[21] Declassification surged in the 1970s. The revelation of secrets from World War II (such as "Ultra" code breaking) produced a spate of historical studies.[22] More theoretical works capitalized on these and on information about cold war intelligence activities that started to become available with the congressional investigations of 1975–76, as well as on ideas from psychology and organizational sociology.[23] The subject sustained two new journals: *Intelligence and National Security* and *The International Journal of Intelligence and Counterintelligence*.

The bulk of research in the second cycle remained preoccupied with how to prevent World War III. (Lessons were often sought by revisiting World War I.)[24] New empiricism corrected prevalent assumptions about policy that had been inferred from deductive theories of deterrence. Scholars who burrowed into declassified documents and interviews revealed that much conventional wisdom among civilians about nuclear targeting did not in fact reflect strategy in practice—the doctrine embodied in the military's Single Integrated Operational Plan for nuclear war. ("Counterforce" targeting, which mainstream theory and political leaders' rhetoric had rejected as destabilizing, had never been abandoned.)[25] Others showed that much of the fundamental logic of

[21] Wohlstetter, *Pearl Harbor* (Stanford: Stanford University Press, 1962); Knorr, "Failures in National Intelligence Estimates," *World Politics* 16 (April 1964).

[22] J. C. Masterman, *The Double-Cross System* (New Haven: Yale University Press, 1972); R. V. Jones, *The Wizard War* (New York: Coward, McCann and Geoghehan, 1978); F. H. Hinsley et al., *British Intelligence in the Second World War*, 5 vols. (New York: Cambridge University Press, 1979–90); Ernest May, ed., *Knowing One's Enemies* (Princeton: Princeton University Press, 1984); Wesley Wark, *The Ultimate Enemy* (Ithaca, N.Y.: Cornell University Press, 1985); Michael Handel, ed., *Strategic and Operational Deception in the Second World War* (London: Cass, 1987); Barry Katz, *Foreign Intelligence* (Cambridge: Harvard University Press, 1989).

[23] Harold Wilensky, *Organizational Intelligence* (New York: Basic Books, 1967); Michael Handel, "The Yom Kippur War and the Inevitability of Surprise," *International Studies Quarterly* 21 (September 1977); Richard Betts, "Analysis, War, and Decision," *World Politics* 31 (October 1978); Lawrence Freedman, *U.S. Intelligence and the Soviet Strategic Threat*, 2d ed. (Princeton: Princeton University Press, 1986); Ariel Levite, *Intelligence and Strategic Surprises* (New York: Columbia University Press, 1987); Richard K. Betts, "Surprise, Scholasticism, and Strategy," *International Studies Quarterly* 33 (September 1989); Ephraim Kam, *Surprise Attack* (Cambridge: Harvard University Press, 1988); Loch Johnson, *America's Secret Power* (New York: Oxford University Press, 1989); Arthur Darling, *The Central Intelligence Agency* (University Park: Pennsylvania State University Press, 1990; James Wirtz, *The Tet Offensive* (Ithaca, N.Y.: Cornell University Press, 1991).

[24] *International Security* 19, special issue (Summer 1984).

[25] Aaron Friedberg, "A History of U.S. Strategic 'Doctrine'—1945–1980," *Journal of Strategic Studies* 3 (December 1980); Desmond Ball, "U.S. Strategic Forces," *International Security* 7 (Winter

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canonical theories about nuclear "stability" that academics and civilian policymakers had come to take for granted was utterly confounded by the realistic operational limits of command and control systems.[26]

The other main strand of empirical work was in conventional strategy. This shift in attention was prompted entirely by the nuclear impasse. None of the convoluted theorizing about how to use or threaten to use nuclear weapons had managed to discover a consistently rational solution to the contradiction between the aims of stabilizing mutual nuclear deterrence between the superpowers and deterring a Soviet conventional attack against NATO. The former required that any nuclear first strike would be suicidal and therefore unthinkable; the latter required that an attack by enemy conventional forces could be blocked without nuclear escalation. Conventional wisdom in the West held that NATO's nonnuclear defenses were too weak and required reliance on the threat of nuclear first use—which meant that it must not be unthinkable. This in turn prevented Washington and Moscow from accepting any hint of inferiority in their respective nuclear forces. If nuclear competition was to be dampened, more confidence in conventional alternatives would be the price.

A new generation of analysts focused on assessing whether, why, and how NATO could achieve more such confidence, by examining in detail the data and assumptions behind standard estimates of the balance of forces and strategic alternatives in Europe. Questioning official assumptions, models, and calculations, and applying new conceptual frameworks, they took up where McNamara's whiz kids had left off in the mid-1960s but approached the problem in more depth. Theoretically, they transposed the concepts and categories of nuclear deterrence theory, whereby particular configurations of forces and emphases in operational doctrine were alleged to foster stability.[27]

1982–83); David Rosenberg, "The Origins of Overkill," *International Security* 17 (Spring 1983). Scott Sagan corrects both the "expert's myth" that nothing changed at all and the "layman's myth" that mutual assured destruction represented U.S. strategy. Sagan, *Moving Targets* (Princeton: Princeton University Press, 1989).

[26] The definitive work is Bruce Blair, *Strategic Command and Control* (Washington, D.C.: Brookings Institution, 1985). See John Steinbruner, "Beyond Rational Deterrence," *World Politics* 28 (January 1976); idem, "National Security and the Concept of Strategic Stability," *Journal of Conflict Resolution* 22 (September 1978); Desmond Ball, *Can Nuclear War Be Controlled?* Adelphi Paper 169 (London: IISS, Autumn 1981); Paul Bracken, *The Command and Control of Nuclear Forces* (New Haven: Yale University Press, 1983); Peter Feaver, *Guarding the Guardians* (Ithaca, N.Y. Cornell University Press, 1992).

[27] Jack Snyder, *The Ideology of the Offensive* (Ithaca, N.Y.: Cornell University Press, 1984); Stephen Van Evera, "The Cult of the Offensive and the Origins of the First World War," *International Security* 9 (Summer 1984); George Quester, *Offense and Defense in the International System*, 2d ed. (New Brunswick, N.J.: Transaction, 1988); Steven Miller, ed., *Conventional Forces and American Defense Policy* (Princeton: Princeton University Press, 1986); Richard Betts, "Conventional Deterrence," *World*

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This wave of attention to conventional forces brought new emphasis on comparative analysis of historical cases. Writing primarily in *International Security* and Studies in Security Affairs, a series published by Cornell University Press, scholars sought additional analytic leverage on questions of relative capability to supplement debates about quantitative models of the military balance. New literature investigated political, economic, social, technological, organizational, and doctrinal issues that determined military effectiveness, and thereby focused the academic consideration of the essence of strategy: how to integrate political ends and military means.[28] A Clausewitz revival ensued in the same period, beginning with a new translation of *On War* (to which Brodie contributed a commentary). The classic *Makers of Modern Strategy* was also updated.[29]

Scholars who did this work prospered in the 1980s. Political science departments that had grown blasé about strategy in the period of détente scrambled to build their staffs again as superpower competition reheated, the Vietnam hangover dissipated, and realist conceptions of world politics rebounded. Opposition to identifying security with strategic studies existed all along,[30] but the identification prevailed in academic hiring in this period. A generous supply of fellowships (espe-

Politics 37 (January 1985); William Kaufmann, "Nonnuclear Deterrence" and "The Arithmetic of Force Planning," in John Steinbruner and Leon Sigal, eds., *Alliance Security* (Washington, D.C.: Brookings Institution, 1983); John Leppingwell, "The Laws of Combat?" *International Security* 12 (Summer 1987); Eliot Cohen, "Toward Better Net Assessment," *International Security* 13 (Summer 1988); John Mearsheimer, "Assessing the Conventional Balance," *International Security* 13 (Spring 1989); Joshua Epstein, "The 3:1 Rule, the Adaptive Dynamic Model, and the Future of Security Studies," *International Security* 13 (Spring 1989); Charles Kupchan, "Setting Conventional Force Requirements," *World Politics* 41 (July 1989); Stephen Biddle, "The European Conventional Balance," *Survival* 30 (March–April 1988).

[28] See, for example, Cornell University Press books such as John Mearsheimer, *Conventional Deterrence* (1983); idem, *Liddell Hart and the Weight of History* (1988); Eliot Cohen, *Citizens and Soldiers* (1985); Barry Posen, *The Sources of Military Doctrine* (1984); Stephen Rosen, *Winning the Next War* (1991). Examples of historically oriented analysis included John Keegan, *The Face of Battle* (New York: Viking, 1976); Martin van Creveld, *Supplying War* (New York: Cambridge University Press, 1977); idem, *Command in War* (Cambridge: Harvard University Press, 1985); Richard Betts, *Surprise Attack* (Washington, D.C.: Brookings Institution, 1982); Allan Millett and Williamson Murray, eds., *Military Effectiveness*, 3 vols. (Boston: Allen and Unwin, 1988); Edward Luttwak, *Strategy* (Cambridge: Harvard University Press, 1987); Michael Handel, *War, Strategy and Intelligence* (London: Cass, 1989); Joseph Bouchard, *Command in Crisis* (New York: Columbia University Press, 1991).

[29] Carl von Clausewitz, *On War*, ed. and trans. Michael Howard and Peter Paret (Princeton: Princeton University Press, 1976); Peter Paret, ed., *Makers of Modern Strategy: From Machiavelli to the Nuclear Age* (Princeton: Princeton University Press, 1986); Edward Mead Earle, ed., *Makers of Modern Strategy: Military Thought from Machiavelli to Hitler* (Princeton: Princeton University Press, 1943). See Harry Summers, *On Strategy* (Novato, Calif.: Presidio Press, 1982), which set off an unprecedented wave of attention to Clausewitz in the U.S. Army; and Michael Handel, ed., *Clausewitz and Modern Strategy* (London: Cass, 1986).

[30] Richard Ullman, "Redefining Security," *International Security* 8 (Summer 1983); Joseph Nye and Sean Lynn-Jones, "International Security Studies," *International Security* 12 (Spring 1988); Jessica Mathews, "Redefining Security," *Foreign Affairs* 68 (Spring 1989).

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cially through Harvard's Olin Institute and Center for Science and International Affairs, the Brookings Institution, and arms control centers sponsored by the Ford Foundation) kept up the corps of researchers in politico-military affairs. This renaissance lasted as long as avoiding World War III remained at the top of the real-world agenda.[31]

The end of the cold war turned security studies back to basics: questions about causes of war and peace, effects of the general distribution of power in international relations, economic and ideological influences on patterns of conflict and cooperation, nationalism, and so forth. Academic research on the operational and technical questions that dominated the 1980s stopped almost completely, but more general work on military institutions, history, and strategic issues thrived.[32] In what we may call either the third cycle of post–World War II strategic studies or the first post–cold war phase, research is advancing on civil-military relations, organization theory, arms control, strategic culture, coercion, grand strategy, and other subjects.[33] In contrast to the cold war, when analysis revolved around deterrence and the East-West military balance, no one policy problem dominates the agenda. This makes the enterprise richer than ever. But without the danger of apocalyptic war at the center, the force of the claim to relevance that overrode intellectual skepticism about the field during the cold war has weakened.

[31] Stephen Walt, "The Renaissance of Security Studies," *International Studies Quarterly* 35 (June 1991). For other reviews, see Colin Gray, *Strategic Studies and Public Policy* (Lexington: University Press of Kentucky, 1982); Helga Haftendorn, "The Security Puzzle," *International Studies Quarterly* 35 (March 1991); Neta Crawford, "Once and Future Security Studies," *Security Studies* 1 (Winter 1991).

[32] Brian Downing, *The Military Revolution and Political Change* (Princeton: Princeton University Press, 1992); Charles Kupchan, *The Vulnerability of Empire* (Ithaca, N.Y.: Cornell University Press, 1994); Kimberly Martin Zisk, *Engaging the Enemy* (Princeton: Princeton University Press, 1993); Bruce Porter, *War and the Rise of the State* (New York: Free Press, 1994); Williamson Murray, MacGregor Knox, and Alvin Bernstein, eds., *The Making of Strategy* (New York: Cambridge University Press, 1994); Jeffrey Legro, *Cooperation under Fire* (Ithaca, N.Y.: Cornell University Press, 1995); Richard K. Betts, *Military Readiness* (Washington, D.C.: Brookings Institution, 1995). Two books on crucial military topics would have been blockbusters had they appeared during the cold war, but were little appreciated after it: Barry Posen, *Inadvertent Escalation* (Ithaca, N.Y.: Cornell University Press, 1991); Paul Stares, *Command Performance* (Washington, D.C.: Brookings Institution, 1991).

[33] Chris Demchak, *Military Organizations, Complex Machines* (Ithaca, N.Y.: Cornell University Press, 1991); Scott Sagan, *The Limits of Safety* (Princeton: Princeton University Press, 1993); Robert Kaufman, *Arms Control during the Prenuclear Era* (New York: Columbia University Press, 1990); Emily Goldman, *Sunken Treaties* (University Park: Pennsylvania State University Press, 1994); Elizabeth Kier, "Culture and Military Doctrine," *International Security* 19 (Spring 1995); Alastair Iain Johnston, *Cultural Realism* (Princeton: Princeton University Press, 1996); Robert Pape, *Bombing to Win* (Ithaca, N.Y.: Cornell University Press, 1996); Peter Liberman, *Does Conquest Pay?* (Princeton: Princeton University Press, 1996); Stephen Rosen, *Societies and Military Power* (Ithaca, N.Y.: Cornell University Press, 1996); Robert Art, "A Defensible Defense," *International Security* 15 (Spring 1991); Michael Desch, *When the Third World Matters* (Baltimore: Johns Hopkins University Press, 1993); Peter Feaver, "The Civil-Military Problematique," *Armed Forces and Society* 23 (Winter 1996); Jonathan Mercer, *Reputation and International Politics* (Ithaca, N.Y.: Cornell University Press, 1995).

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Where should strategic studies go? The current trajectory, on which a wider array of research topics rides than during the first two cycles, is a good one. Weak spots in the earlier cycles could still stand more work today: the political dimension of internal or civil war, and the operational dimension of irregular or subconventional war. Since 1945 scholars focused most on interstate war and nuclear or conventional strategy, but most of the conflicts that actually occur are of the other sorts. The comparative politics field in political science attends to internal conflict, and there is plenty of atheoretical policy literature on "low intensity" conflict, but not yet enough academic attention within international relations and strategic studies.[34] Another topic that merits special attention is the evolution of Chinese forces, doctrine, and strategy, and whether China's military development can match its economic surge. The cold war spawned an impressive corps of analysts of the Soviet military (Christopher Donnelly, John Erickson, Mary Fitzgerald, Raymond Garthoff, David Holloway, Arnold Horelick, Roman Kolkowicz, Stephen Meyer, Michael MccGwire, William Odom, Thomas Wolfe, and many others); there are counterparts on China (such as June Dreyer, Paul Godwin, John Lewis, Jonathan Pollack, David Shambaugh, and Arthur Waldron), but the list is shorter.

Despite the widening ambit after the cold war, skeptics who never liked the ascendancy of strategic studies see less reason to indulge it and demand that "security" studies be broadened.[35] The effect of accepting these arguments would be to slash attention to military strategy in universities. The best solution to intellectual controversy is to let a hundred flowers bloom, but departments do not have a hundred flower pots. Few, as it is, have found room for more than one expert on military affairs, and some have none. Broad definitions of security would allow departments to hire specialists in areas far afield from war and strategy and still claim that they cover the security slot.

The Missing Discipline

As Thomas Schelling argued in 1960, strategy's theoretical development has been retarded because "the military services, in contrast to al-

[34] Stephen R. David, "Explaining Third World Alignment," *World Politics* 43 (January 1991); Barry Posen, "The Security Dilemma and Ethnic Conflict," *Survival* 35 (Spring 1993); Chaim Kaufmann, "Possible and Impossible Solutions to Ethnic Civil Wars," *International Security* 20 (Spring 1996).

[35] Edward Kolodziej, "Renaissance in Security Studies?" *International Studies Quarterly* 36 (December 1992); Ronnie Lipschutz, ed., *On Security* (New York: Columbia University Press, 1995); Peter Katzenstein, "Conclusion," in Katzenstein, ed., *The Culture of National Security* (New York: Columbia University Press, 1996); Keith Krause and Michael Williams, "Broadening the Agenda of Security Studies," *Mershon International Studies Review* 40 (October 1996).

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most any other sizable and respectable profession, have no identifiable academic counterpart."[36] Strategic studies has piggybacked on other disciplines—mainly history and political science—instead of securing an autonomous institutional home. There are no *departments* of strategy or war studies in U.S. universities (in contrast to Britain). This in itself is not damning; not all interdisciplinary fields have departmental status. But there is still a disjunction between intellectual and institutional logics. The essence of strategy should be the integration of two disciplines—military science and political science—but one of them is missing. Interdisciplinary strategy suffers from the lack of an established academic *discipline* of military science to anchor it.

First, there is no institutional redoubt to fall back on when support for interdisciplinary work declines. To understand the causes, conduct, and consequences of war, one should know something of politics, economics, psychology, sociology, geography, technology, force structure, and tactics. When world developments favor strategic studies, the interdisciplinary character is an advantage, since it exploits strengths of several fields. Otherwise it is a vulnerability, since enthusiasm for interdisciplinary research falters when making room for it encroaches on one's own department.

Second, specialists in strategy are spread thinner. Unlike political scientists in international political economy (IPE), they have no analogue to economics as an allied field to draw on. They must develop the military science aspects of their work themselves, as autodidacts. (Then they smuggle military science into political science, where colleagues sometimes wonder whether what they are doing belongs there.) IPE does not focus on the technical functioning of markets but can assume that serious students have at least taken a basic course in economics. Strategists must cram the relevant military science into their own teaching, since students do not get it anywhere else. Economics is assumed to be fundamental for education in international affairs, but elementary military science is not. (In my own university's master's program, all students must take three economics courses; none but the few specializing in security policy are required to take any course on military matters.) In a world of limited resources that keeps many claims at bay, none of this means that military science should be a full-

[36] Schelling, *The Strategy of Conflict* (Cambridge: Harvard University Press, 1960), 8. There is also constant debate (as among students of politics) over whether strategy is science or art. Clausewitz and Sun Tzu are usually identified with art and Jomini with science, but Michael Handel shows that there is more agreement among the three than generally assumed. Handel, *Masters of War*, 2d ed. (London: Cass, 1996).

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fledged discipline in the arts and sciences. But without an institutionally established discipline at the core, strategy must either be welcomed into other disciplines as a sideline or exiled from universities.

Academic work on strategy is sponsored elsewhere, primarily the service war colleges and the National Defense University. In "Strategy as a Science," Brodie wrote, "We need to make of our war colleges genuine graduate schools" (p. 487). This aim has been best approximated in the Naval War College, which has a strategy department dominated by civilian historians. These islands within military organizations, however, will never sustain strategic studies on their own, nor should they. Understanding of military affairs should not become a closed system, where none outside the uniformed establishment can claim expertise. If serious strategic studies is to survive, it needs a niche in real universities. Given the interdisciplinary and policy-oriented nature of strategic studies, graduate schools of public policy and international affairs should be a logical locus. But although such schools now give Ph.D.'s, they do not have an autonomous underpinning. Scholars staffing them still come mostly from the arts and sciences, which is where a viable academic enterprise must have roots.

Most social sciences have dealt with military subjects. For example, sociologists such as Morris Janowitz and Charles Moskos built the Inter-University Seminar on Armed Forces and Society and its journal *Armed Forces and Society*. Few of the social sciences, however, have generated a critical mass of scholars fully conversant with strategy. Economists got involved in strategic work as consultants, or by moving to RAND or Washington, not by establishing it as a field within their parent discipline. In practice, history and political science are the homes for strategic studies.

Military history is essential knowledge for anyone prescribing strategy, but it does not fare well within the history profession. Few major departments beyond Yale, Duke, and Ohio State have kept even a single military historian on their rosters. Strategic studies has been more welcome in political science. In part this is because political science has been an eclectic and permissive discipline, without a rigidly autonomous agenda, method, or qualifications. (Several major political science departments even appointed faculty with other degrees to cover strategy—mathematician Albert Wohlstetter at the University of Chicago, chemist George Rathjens at MIT, and economist Robert Powell at Berkeley. It is hard to imagine departments in any of those other disciplines hiring a political scientist for anything.) Most researchers in international relations exploit other disciplines for much of their work.

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Empirically oriented ones look to history, those interested in deductive theory look to economics and philosophy. The most zealous social scientists, however, see methodological eclecticism as flabby indiscipline. Strategy will not fare well if scientistic impulses achieve hegemony in the political science guild. Strategic studies can and should be as rigorous as any discipline, but it has a natural interest in a permissive writ for political science, if that discipline is to be its main home.[37]

Lack of a military science discipline also limits institutional links between military and academic cultures. Both camps have come a long way since "Strategy as a Science." Officers have become civilianized, getting M.B.A.'s or social science Ph.D.'s, complementing the traditional military orientation to engineering; and civilians have become militarized, serving in the Defense Department or getting more rounded educational backgrounds in military operations than McNamara's systems analysts had. Blurry boundaries among the realms of policy, strategy, and operations, however, keep the proper balance of civil-military power uncertain. Most accord civilians the right to make policy, and the military the right to run operations, but strategy is what links the two. Pulled in two directions, strategic choices are inevitably seen by some as primarily political and civilian and by others as primarily operational and military.[38]

Brodie did not consider the professional military equipped to accomplish the integration of policy and operations that is the essence of strategy, but American society would really not have it otherwise. Strategy sucks the military into high politics. Professional soldiers usually prefer a division of labor, segregating policy and operations into neat compartments, assuming that strategy will be their own mechanistic translation of policy guidance into military programs and plans that they can execute to the stipulated ends. Civilian strategists worry that military tunnel vision may yield dangerous and unrecognized political consequences—for example, building incentives for preemptive attack into the configuration of capabilities.

If strategy is to integrate policy and operations, it must be devised not just by politically sensitive soldiers but by militarily sensitive civilians. Either of these types makes third parties in politics or academia uncomfortable. Ironically, many academics who endorse strong civilian control of the mili-

[37] A dean of the profession's brief for eclecticism is Gabriel Almond, "Separate Tables," *PS* 23 (Fall 1988), 829–30, 839–40.

[38] Samuel Huntington, *The Soldier and the State* (Cambridge: Harvard University Press, 1957); Samuel Finer, *The Man on Horseback* (New York: Praeger, 1962); Amos Perlmutter, *The Military and Politics in Modern Times* (New Haven: Yale University Press, 1977); Richard K. Betts, *Soldiers, Statesmen, and Cold War Crises*, 2d ed. (New York: Columbia University Press, 1991).

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tary prove reluctant to support it by promoting civilian strategic studies. Amateurs should not control what they do not understand, especially in a business that puts legions of lives at stake. Yet many academic critics share military skepticism (albeit for different reasons) about intellectual attention to details of military operations.

The main problem is not the pacifist or radical fringes of the academic world, despite the distaste they evince for a field they associate with support for U.S. policy. Neither group has as much clout in political science as elsewhere in academia. The problem is that many in the liberal mainstream concede that strategic studies is legitimate, but when major war appears to recede as a prospect in the real world—as it did in the 1970s and again after the cold war—they resist ranking the subject highly when their own fields' priorities are at stake. Seen as legitimate in principle, strategic studies faces marginalization in practice when departments see it as a second-rate claim on their discipline.

Strategic Studies and Security Studies

The intellectual and institutional status of strategy is confused by persistent lack of consensus on how much attention military aspects of security should get and where lines should be drawn between narrow military science, integrative strategic studies, and all-encompassing security studies. In "Strategy as a Science" Brodie noted that military strategy was subordinate to the larger problem of how

> to increase one's advantage without unduly jeopardizing the maintenance of peace or the pursuit of other values. This broader enterprise, which might be called "security policy," can be construed to cover the total preparation for war as well as the waging of it. It would thus deal . . . with political, social, and economic as well as military matters in both domestic and foreign contexts. (p. 477)

Brodie's "security policy" was closer to what I have called strategic studies, as his discussion of "strategy" was closer to military science, perhaps because he did not foresee vigorous arguments that security involves far more than preparation for war. His later frustration with economists' approach to strategy was their inattention to factors he lumped with "security" in 1949. Today it is fair to distinguish strategic and security studies in order to recognize that security includes things besides military concerns, as long as no doubt is left that security policy requires careful attention to war and strategy. Security studies today embraces many related topics such as diplomacy, policy formation, social and

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economic mobilization, scientific innovation, arms control, and terrorism.[39] Some, however, regard even this breadth as inadequate.

As semantic commentary on the *term* "security," arguments that security studies should consider problems ranging from economic performance to environmental damage are quite fair. They do not help to organize the field of international relations, however, because they do not delimit a subfield. A subfield must be broad enough to encompass a significant range of problems, but narrow enough to be a coherent area of inquiry, distinguishable from other subfields and the parent field. Expansive definitions of security quickly become synonymous with "interest" or "well-being," do not exclude anything in international relations or foreign policy, and thus become indistinguishable from those fields or other subfields. Recognition of this boundary problem led Baldwin to suggest that security studies be abolished as a subfield and "reintegrated" into international relations. If the point was to reverse fragmentation and encourage the reintegration of all specializations, this argument would be reasonable, but he denies that the other main subfield of international relations, IPE, should be reintegrated as well.

First, Baldwin argues, no other subfield but security is "defined in terms of techniques of statecraft."[40] Even if this is true, the difference is less significant than the similarities. IPE is as much or as little about economic phenomena as security studies is about military phenomena. Both trade and war involve conflict and cooperation, negotiation, and ultimate media of exchange and settlement (cash payment and combat). Both combat and commerce are modes of interaction in which purposes, constraints, instruments, and procedural dynamics produce outcomes and overlap with other realms of interaction.

Second, Baldwin suggests, "the rationale for subfields is to ensure that important subtopics are not neglected,"[41] and security topics are established at the core of the parent field of international relations where realism is the dominant paradigm. Specialization, however, is at least as much for *deepening* knowledge on important subjects as for

[39] Examples of breadth beyond strictly military aspects include Paul Kennedy, *The Rise and Fall of the Great Powers* (New York: Random House, 1987); Aaron Friedberg, *The Weary Titan* (Princeton: Princeton University Press, 1988); Richard Ullman, *Securing Europe* (Princeton: Princeton University Press, 1991); Jack Snyder, *Myths of Empire* (Ithaca, N.Y.: Cornell University Press, 1991); Douglas Macdonald, *Adventures in Chaos* (Cambridge: Harvard University Press, 1992); Stephen Walt, *The Origins of Alliances* (Ithaca, N.Y.: Cornell University Press, 1987); idem, *Revolution and War* (Ithaca, N.Y.: Cornell University Press, 1995).

[40] Baldwin (fn. 3), 140. See also Baldwin, "The Concept of Security," *Review of International Studies* 23 (1997).

[41] Baldwin (fn. 3), 140.

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guarding against neglect. Moreover, it has been twenty years since one could worry that IPE might be neglected, and realism has been on the defensive again since the cold war ended. Considering that international relations has more or less broken down into two main subfields, it hardly seems necessary to drop to one. If anything, more subfields should be strengthened (for example, environmental studies, which covers subjects ultimately as important as the regnant subfields and is more neglected than either security or IPE).

Clarity and claims might best be served by renaming the security subfield "IPM" (international politico-military studies). This would confirm the focus on strategic integration of ends and means, highlight the parallel to IPE, and circumvent the dispute over "security" that mixes legitimate semantic claims with objectionable attacks on strategic studies. The deal would concede the case for identifying the scope of security with international relations in general, in exchange for recognition of an "IPM" subfield (strategic studies) on a par with any other. Practically, however, there is no constituency on either side for such recategorization, so strategy's academic status will continue to be set through arguments about security studies.

As consensus on standards remains elusive, students of strategy regularly encounter criticisms of the field's quality, occasionally in print but most often in professional badinage. One objection is that mainstream strategic work is theoretically weak or has not advanced since the deterrence theory of the early cold war.[42] John Ruggie laments failures to consider possible transformations of international politics: "the worst offender by far is the American field of security studies," because "no epochal thought has been expressed by any serious specialist in that field since 1957, when John Herz published 'Rise and Demise of the Territorial State.'"[43] This confuses disagreement with closed minds: there is no evidence that those who disbelieve in transformation have refused to consider it, any more than that those Ruggie admires have refused to consider the case for continuity.

Have other subfields done much better in producing knowledge? Not by standards of cumulation or cross-fertilization. Work on deterrence and arms control represented as much cumulation as found in most of political science. Indeed, if work in the later cold war amounted to refinements of earlier breakthroughs rather than new ones, this represented progress based on cumulation. Debates on war causation and civil-military relations have filtered into other subfields via levels-of-

[42] Nye and Lynn-Jones (fn.30), 12, 21–22, 26.
[43] Ruggie, "Territoriality and Beyond," *International Organization* 47 (Winter 1993), 143.

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analysis and bureaucratic politics arguments, and security studies adapted cognitive theory and organization theory before IPE did.[44]

Even if it were true that theoretical innovation in strategic studies has been less paradigm-shattering than in other fields, this would not ipso facto demonstrate weakness rather than strength. Critics would have to demonstrate that more recent and numerous theories in other fields are *better* theories—more useful for understanding the world—than the fewer and older ones of strategy. Theories may endure because they prove durable, or may change constantly because each new one proves wanting. One Clausewitz is still worth a busload of most other theorists.

Are technical discussions about weaponry or operational doctrine evidence of strategists' atheoretical fixation on particulars? Such criticism has some merit in regard to technically denominated literature of the cold war (though most of it was not in political science) and is understandable when provoked by hardware fetishists often taken for representatives of strategic studies. Otherwise, it is no more reasonable than it would be to denigrate political economy for attention to specific commodities, financial instruments, or trade agreements.

Some critics such as rational choice theorists who deride traditional empirical work as "just telling stories," or quantitative researchers who criticize it as "anecdotal," see emphasis on comparative case studies as generically weak compared to deductive theorizing or "large-N" studies. These other approaches thrive and compete effectively in universities with mainstream strategic studies as practiced in Brodie's tradition. Such work appears mainly in *The Journal of Conflict Resolution*, *American Political Science Review*, and *International Studies Quarterly*.[45]

Distaste for military studies sometimes comes from moral suspicions that it embraces war rather than attending to how to abolish it. American strategic research, however, is mainly about how to avoid war. Most work in strategic studies is profoundly *conservative*, in the literal sense, because it is concerned with *stability*, a value that privileges peace

[44] Thanks to Peter Feaver for this point. See John Steinbruner, *The Cybernetic Theory of Decision* (Princeton: Princeton University Press, 1974); Robert Jervis, *Perception and Misperception in International Politics* (Princeton: Princeton University Press, 1976); Graham Allison, *Essence of Decision* (Boston: Little, Brown, 1971); Harvey Sapolsky, *The Polaris System Development* (Cambridge: Harvard University Press, 1972).

[45] See also Bruce Bueno de Mesquita, *The War Trap* (New Haven: Yale University Press, 1981); Bueno de Mesquita and David Lalman, *War and Reason* (New Haven: Yale University Press, 1992); Jack Levy, *War and the Modern Great Power System, 1495-1975* (Lexington: University Press of Kentucky, 1983); Paul Huth, *Extended Deterrence and the Prevention of War* (New Haven: Yale University Press, 1988); Robert Powell, *Nuclear Deterrence Theory* (New York: Cambridge University Press, 1990); George W. Downs and David M. Rocke, *Tacit Bargaining, Arms Races, and Arms Control* (Ann Arbor: University of Michigan Press, 1990).

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over revisionism. In this respect, liberals interested in arms control have been the most conservative. Few academic works promote schemes for using force to change the status quo. Rather, they focus on deterrence or defense, to discourage the resort to violence to effect political change.

Focusing intently on how to manipulate the threat of deadly force, for whatever benign purpose, strikes some as fatalistic, selling short the search for cooperative strategies. Why waste time and foundation grants on finding better ways to do a bad thing when we might apply our talents to making it unnecessary? But accepting the occurrence of war and considering how to cope with it more effectively are no more fatalistic than accepting liberal capitalism, and considering how to optimize trade within it, would seem to a Marxist. Realist assumptions about group conflict that underlie most strategic studies require no more and no less validation than those of optimists who believe in the obsolescence of war. Debate over these assumptions lies at the heart of political theory and has been recycled and unresolved for centuries. It would be foolhardy to bet that social science can resolve it and arrogant for either side to deny an academic place to the other.[46]

Strategy for What?

Are scholars of strategy too policy-oriented (not sufficiently theoretical) or too involved in government consulting to keep straight the conflicting demands of truth and power? (At different times, critics have given it both ways—denigrating the field for being too relevant in the era when there were huge security problems and dismissing it now for not being relevant enough.) At high points of the cold war, analysis often did fixate on the U.S.-Soviet balance of military power and the relative merits of particular weapons programs. It is also true that few strategists apologize for wanting to affect prospects for war and peace in the outside world. Apart from aesthetic fascination with the elegance of theory itself—theory for theory's sake—the rationale for valuing theoretical over policy analysis in the intellectual pecking order is that the former can subsume and inspire a wider range of analysis, and thus reveals more and lasts longer than work on a transient issue. This utilitarian rationale means that one good theory can illuminate many policy questions—but also that some link between theory and practice is ultimately the test of a theory's value. *Neither theory nor policy can be optimized apart from each other.* Central theoretical insights often flow from

[46] See Christopher Davis, "War and Peace in a Multipolar World," *Journal of Strategic Studies* 19 (March 1996).

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grappling with concrete questions rather than a priori constructs. For example, Albert Wohlstetter drew basic precepts about strategic instability from his work on a RAND study of choices in bomber deployment patterns.[47]

Two academic pathologies should raise the stock of policy studies. One is that the professional premium on theorizing tends to proliferate theories, promote constant revision of theories, and encourage production of second-rate theories over first-rate applications. Albert Hirschman, with impeccable credentials as a theorist, long ago indicted "the tendency toward *compulsive and mindless theorizing*."[48] One sure sign of intellectual degeneration in a field is when the logical relationship between generalization and specification is inverted, theories threaten to outnumber their applications, and the shelf life of theoretical work turns out to be hardly longer than that of policy analysis. Some social scientists are untroubled that professional incentives encourage such imbalance, because never having had to meet a payroll in the policy world, they overestimate the ease with which an effective application can be derived from a theoretical insight. Every intellectual would rather be an Einstein than an engineer, but useful knowledge is not advanced if the academy generates a horde of would-be Einsteins but few competent engineers. Strategists are not just engineers, but they consider empiricism and application no less important than the theoretical part of their work.

The other pathology is when theorization becomes a closed system, with no connection through which insights can be applied to the outside world—when theorists communicate effectively with no one but each other. When this happens, a theory may remain beautiful but it loses the claim to utility. It is the widespread perception in the outside world that theorization is a closed system that makes "academic" a pejorative adjective in normal parlance. A system can be closed in two senses: lack of feedback from policy application, or lack of interest in testing theories against evidence. Both problems are addressed in typical strategic studies research programs that proceed from policy issues, to theoretical formulation, to empirical testing, to policy application.

Intellectuals who spend much time in Washington sometimes worry that much theoretical work in contemporary political science reflects both pathologies and has not proved much less ephemeral or more useful than good applications of old theory. Unless academics themselves

[47] Wohlstetter, "The Delicate Balance of Terror," *Foreign Affairs* 37 (January 1959).

[48] Hirschman, "The Search for Paradigms as a Hindrance to Understanding," *World Politics* 22 (April 1970), emphasis in original.

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become involved on the periphery of policy-making, the only way that their work can have effect outside the closed system in universities is if practitioners read it. Few high-level staff in the U.S. government read anything more academic than *Foreign Affairs*, and high-level policymakers seldom have time to read any unofficial material but op-ed pieces. One academic journal that *is* read occasionally in Washington is *International Security*, because it melds policy analysis and theory. This is one reason it has had a circulation 50 to 80 percent higher than its IPE counterpart *International Organization* and that academics in other fields sometimes denigrate its academic quality.

Some academics may value the aesthetic qualities of theory as much as the utilitarian. Strategists can get as excited as anyone over the elegance of an idea, but see elegance without empirical confirmation and applicability as no more science than art. As Brodie suggested, any criterion for strategy but a utilitarian one is a contradiction in terms: "The question that matters in strategy is: Will the idea work? . . . . *Strategy is a field where truth is sought in the pursuit of viable solutions.*"[49]

In the first half of the cold war, academic strategists played a visible role in U.S. defense policy. There have been many officials with Ph.D.'s since. For better or worse, however, few practicing academics in strategic studies have been directly influential since the 1960s, except for Henry Kissinger. Most scholars who have held high national security offices have been generalists (McGeorge Bundy, Walt Rostow, Zbigniew Brzezinski) or ones from strategic studies who left academia early in their careers (Michael Armacost, Paul Wolfowitz, Arnold Kanter, Dennis Ross, Lynn Davis). Some academic strategists are consultants to foreign affairs agencies, but few are reputed to be powers behind any throne. Ironically, in the past quarter century, policy experience has enriched academic research more than the reverse, since many prominent scholars of strategy spent brief periods early in their careers working at middle levels in the government (usually thanks to fellowships from the Council on Foreign Relations).

The direct effect of strategic studies on the outside world may be greater than that of much other social science. It remains quite limited, however, perhaps because scholarship in the field became more academic after the first cycle. Thus the field is more like others than it is different, in the sense that the influence of education is hard to pinpoint. It percolates through students who go into the outside world, through the few policymakers who read research, or through other channels dif-

[49] Brodie, *War and Politics* (fn.4), 452–53, emphasis in original.

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ficult to trace. In any case, to whatever extent strategic studies is not a closed system, it is cause for celebration, not criticism.

Brodie's disappointment with the first cycle reflected the failure of strategists then most prominent to integrate the analytic rigor of economics with the broader expertise in military science, politics, and history that he himself had. Strategic studies improved in those terms in the second cycle. Now the question is whether strategic studies, larded as it is with military science, will remain at the center of security studies or will wither as academic guilds drive the focus of research to other subjects.

Strategy is not the whole of security and need not be anointed as the first priority of international relations. This defense of strategic studies is not a special pleading to return the field to a dominant position, but simply a case for keeping its status equal to any other subfield. Whatever resources are available for hiring, faculties should decide what to cover on the basis of long-term evidence of what has mattered in world politics rather than recent events, intellectual fads, or moral hopes. A department that can afford only one professor of international relations needs a generalist and cannot demand that she know much military science. A department that can hire in separate subfields, however, should ensure that coverage of "security" includes as much emphasis on strategic studies as if the slot were defined as in "IPM."

War has always been an essential phenomenon in world politics. There is nothing wrong with asserting that it is waning as long as such propositions (which have been popularized and discredited three times before in the past century) are not allowed to strike the issue from the agenda of highest priority problems. If war does become obsolete, the wasted intellectual effort in continuing to study it will have been a small price. If it does not, and if research ever has any useful impact at all, future generations may be glad that we kept our intellectual powder dry.

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THEORETICAL DECAY AND THEORETICAL DEVELOPMENT
The Resurgence of Institutional Analysis

By KAREN L. REMMER*

THE publication of Samuel P. Huntington's article, "Political Development and Political Decay," in *World Politics* marked a critical turning point in the study of comparative politics.[1] The article appeared just as the expansion of interest in the politics of non-Western states had begun to spawn a rapidly growing body of literature on modernization and political development—a literature notable less for the rigor of its theoretical specifications or the robustness of its research findings than for the proliferation of abstract, complex, and value-laden concepts. Huntington not only offered a pioneering critique of these tendencies; by reconceptualizing political development as institutionalization, he attempted to steer research on Third World politics in new and more productive directions.

Despite its innovative and constructive agenda, in many respects "Political Development and Political Decay" represented the culmination of a relatively unproductive line of research rather than the first step toward the inception of a new theoretical project oriented around the study of political institutions. Through his critique of the development literature, Huntington paved the way for the diversification of analytical approaches to the study of less developed regions of the world. By the early 1970s, new concepts and theoretical frameworks, including dependency, corporatism, and bureaucratic authoritarianism, had all but totally eclipsed the study of political development. Institutions, however, remained far from the center of political analysis. Thirty years later, we appear to have come full circle. Although no one writes of development or decay, comparativists have rediscovered political institu-

* The author wishes to thank William Stanley, Neil Mitchell, and Gilbert W. Merkx for their helpful comments on an earlier draft of this article. Thanks are also due to Michael Ballard for research assistance.

[1] Samuel P. Huntington, "Political Development and Political Decay," *World Politics* 17 (April 1965).

World Politics 50 (October 1997), 34–61

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tions, emphasizing the originality of Huntington's 1965 contribution and redirecting our attention to the potential variability of political outcomes in the face of sweeping global currents.

Modernization and Political Development

The pivotal challenge confronted by Huntington in "Political Development and Decay" was that of reformulating the broad, multidimensional, and optimistic models of modernization and development theorists in ways that would make it possible to address ongoing patterns of political change. Looking at the less developed regions of the globe, Huntington observed a fundamental gap between theory and reality:

> Instead of a trend toward competitiveness and democracy, there has been an "erosion of democracy" and a tendency to autocratic military regimes and one-party regimes. Instead of stability, there have been repeated coups and revolts. Instead of a unifying nationalism and nation-building, there have been repeated ethnic conflicts and civil wars. Instead of institutional rationalization and differentiation, there has frequently been a decay of the administrative organizations inherited from the colonial era and a weakening and disruption of the political organizations developed during the struggle for independence. (p. 392)

This "open your eyes to reality" message called into quesion the relevance of prior research and redefined the central puzzle facing students of Asian, African, and Latin American politics as political decay (or disorder) rather than development.

To address this puzzle, Huntington distinguished political development from modernization and identified the former with institutionalization, which he defined as "the process by which organizations and procedures acquire value and stability" (p. 394). The implications were far-reaching. By severing the concept of development from modernization, Huntington discarded the evolutionary, unidirectional, and teleological assumptions inherited from nineteenth-century European theorists, such as Ferdinand Toennies and Max Weber, and opened the door to meaningful empirical inquiry about the relationship between socioeconomic change and political outcomes. If development and modernization represented distinct sets of social processes, the two might be related positively, negatively, or not at all. Hence, however inexorable the global process of modernization or Westernization, political development was a potentially reversible outcome that might vary significantly across time and space. "Political Development and Political Decay" thereby created the basis for a fundamental rethinking of development theory.

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In Huntington's formulation, the relationship between modernization and development was negative or conflictual. He asserted, "Rapid economic growth breeds political instability" (p. 406). Likewise, he saw the expansion of literacy, education, urbanization, industrialization, communication, and virtually every other aspect of the modernization process as threatening to political order. Central emphasis, however, was placed upon rapid increases in social mobilization and political participation, which Huntington described as "directly responsible for the deterioration of political institutions" (p. 405). The argument built upon the work of William Kornhauser[2] in identifying rapid social change with the breakdown of mechanisms for controlling mass pressures on the political system. Stability might be maintained by insulating political institutions from popular pressures, either by slowing down the process of social mobilization or by restricting competition among elites for popular support. The alternative involved constructing new political institutions capable of weathering the forces of modernization. In Huntington's view, only political parties could play this role: "Where traditional political institutions are weak or non-existent, the prerequisite of stability is at least one highly institutionalized political party" (p. 425).

Quite apart from its ideological biases and cold war overtones, Huntington's attempt to recast development theory was less than successful. His key concepts were too broad to be analytically useful and were defined in ways that rendered the central line of argument all but immune to falsification. Political decay encompassed phenomena ranging from mass-based revolutions and ethnic conflicts to elitist coup conspiracies, whose causes and consequences presumably differ. Likewise, modernization was defined in terms of a complex and potentially uncorrelated array of political, economic, and social variables, whose relationship to the process of political institutionalization might depend upon whether one was exploring the impact of economic growth, political participation, or some other independent variable, or focusing on labor unions, revolutionary organizations, traditional kingship, the military, or some other kind of institution. Since political development was defined largely in terms of institutional stability, the central argument boiled down to the idea that change (or modernization) undermines stability (or institutionalization).

If the result was less than precise, powerful, or even testable theory, Huntington nevertheless helped to push the development literature in that direction. Unlike most of his contemporaries, Huntington was not interested in understanding political change merely in terms of phases,

[2] William Kornhauser, *The Politics of Mass Society* (New York: Free Press of Glencoe, 1959).

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conceptual schemes, or ideal types. He reconceptualized political development in order to incorporate that concept within a theoretical framework capable of elucidating the question of political instability. In the process, he redefined the central issues in ways that exposed the limitations of the political development literature and rendered its central concept largely superfluous. By 1968, when Huntington elaborated upon the ideas presented in "Political Development and Political Decay" in a book-length study, he himself had all but jettisoned the concept of political development in favor of "political order."[3] This "conceptual housecleaning"[4] not only set the stage for a dramatic expansion of empirical research on political instability,[5] but also created room for the emergence of new theoretical approaches to the study of the less industrialized regions of the globe.

From Development to the State

Although scholars continued to write about political development in an idealist vein through the mid-1970s, Huntington's critique of the preexisting literature was joined by two other major lines of theoretical analysis. First, building on new empirical findings about politics in the developing nations as well as on comparative historical studies of the developed world, scholars began to emphasize the heterogeneity of tradition as well as its resilience to the forces of modernization.[6] Simplis-

[3] Samuel P. Huntington, *Political Order in Changing Societies* (New Haven: Yale University Press, 1968). In a coauthored essay on political development that appeared some years later, Huntington and Jorge I. Domínguez observed that "It was . . . once argued that political development should be defined as political institutionalization. On subsequent reflection, however, there did not seem to be much to be gained by trying to identify one rather diffuse, controversial, and value-laden concept (like political development) with a somewhat more specific, analytical, and value-free concept (like institutionalization)." Huntington and Dominguez, "Political Development," in Fred I. Greenstein and Nelson W. Polsby, eds., *Handbook of Political Science*, vol. 3 (Reading, Mass.: Addison-Wesley, 1975), 47.

[4] This concept is drawn from Sigmund Neumann, "Comparative Politics: A Half-Century Appraisal," in Louis J. Cantori and Andrew H. Ziegler Jr., eds., *Comparative Politics in the Post-Behavioral Era* (Boulder, Colo.: Lynne Rienner, 1988), 17.

[5] See, for example, Ernest Duff and John McCamant, *Violence and Repression in Latin America: A Quantitative and Historical Analysis* (New York: Free Press, 1976); Ivo K. Feierabend, Rosalind L. Feierabend, and Ted Robert Gurr, eds., *Anger, Violence and Politics* (Englewood Cliffs, N.J.: Prentice-Hall, 1973); Ted Robert Gurr, *Why Men Rebel* (Princeton: Princeton University Press, 1970); Douglas A. Hibbs, *Mass Political Violence: A Cross-National Causal Analysis* (New York: John Wiley and Sons, 1973); Robert D. Putnam, "Toward Explaining Military Intervention in Latin American Politics," *World Politics* 20 (October 1967); Claude E. Welch Jr. and Arthur K. Smith, *Military Role and Rule* (North Scituate, Mass.: Duxbury Press, 1974).

[6] Seminal works in this vein include Lloyd I. Rudolph and Susanne Hoeber Rudolph, *The Modernity of Tradition: Political Development in India* (Chicago: University of Chicago Press, 1967); Reinhard Bendix, "Tradition and Modernity Reconsidered," *Comparative Studies in Society and History* 9 (April 1967); S. N. Eisenstadt, *Tradition, Change, and Modernity* (New York: John Wiley and Sons, 1973); Joseph R. Gusfield, "Tradition and Modernity: Misplaced Polarities in the Study of Social Change," *American Journal of Sociology* 72 (January 1967); Barrington Moore, *The Social Origins of Dictatorship and Democracy* (Boston: Beacon Press, 1966).

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tic dichotomies between traditional and modern societies thereby gave way to more nuanced frameworks emphasizing differences rather than similarities in development paths across both time and space. The concept of political culture figured prominently in this line of analysis.

The second major set of objections to the development paradigm emphasized the diversity of the modernization process itself.[7] If the timing, pace, sequence, and agents of modernization varied across societies, unilinear theories of development were clearly untenable. Just as differences among traditional societies influenced the outcome of modernization processes, differences in the process of modernization might also be expected to yield dissimilar results across nations and regions of the world. The dominant analytical tendency associated with this second line of analysis was structuralist in nature, and its most significant product, the dependency literature.

Both of these major lines of criticism followed Huntington in discarding unilinear assumptions about political change. The countries of the Third World might be modernizing, but not necessarily in the same way as North Atlantic societies. Development simply could not be treated as a synonym for Western history. The optimistic equations of development and modernization theory were thus displaced by a growing consensus that ongoing processes of change in Asia, Africa, or Latin America were unlikely to establish the basis for capitalist democracy or otherwise replicate the experience of the advanced industrialized nations. The reasons for this expectation varied considerably, however, provoking a prolonged debate over the relative importance of cultural and structural forces in the determination of macropolitical outcomes—a debate that overshadowed earlier literature and largely bypassed the institutional level of analysis highlighted in Huntington's seminal article.

In emphasizing the role of values, cultural approaches built heavily on the modernization literature. But whereas the latter assumed that education, urbanization, industrialization, and other aspects of the modernization process would promote modern values and the development of a "civic culture,"[8] revisionists regarded traditional values as re-

[7] Influential contributions include A. Gerschenkron, *Economic Backwardness in Historical Perspective* (Westport, Conn.: Greenwood Press, 1966); Raúl Prebisch, "Development Problems of the Peripheral Countries and the Terms of Trade," in J. D. Theberge, ed., *Towards a Dynamic Development Policy in Latin America* (New York: United Nations, 1963); Karl de Schweinitz, *Industrialization and Democracy: Economic Necessities and Political Possibilities* (New York: Free Press of Glencoe, 1964).

[8] Seminal works include Gabriel Almond and Sidney Verba, *The Civic Culture* (Boston: Little, Brown, 1965); Alex Inkeles, "Making Men Modern: On the Causes and Consequences of Individual Change in Six Developing Countries," *American Journal of Sociology* 75 (September 1969); Alex Inkeles and David H. Smith, *Becoming Modern* (Cambridge: Harvard University Press, 1974); Daniel

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sistant to change. The origins of military coups, ethnic strife, and other phenomena that had been emphasized by Huntington were thus sought not in the destructive impact of modernization, but in cultural traditions that interacted with the forces of modernization to generate widely varying development outcomes. Titles such as *The Politics of Tradition: Continuity and Change in Northern Nigeria, 1946–1966, Politics and Social Change in Latin America: The Distinct Tradition, Culture and Politics in Indonesia,* and *The Centralist Tradition of Latin America*[9] thus began to displace such influential works as *The Politics of Modernization, The Politics of the Developing Areas,* and *The Passing of Traditional Society*[10] as well as the related body of monotonously titled regional and case-study literature.[11]

Working from the modern rather than traditional side of the question, structural theorists argued that the type of modernization experienced in the less developed regions of the world differed significantly from that in the North Atlantic. The most influential of these writings drew on the concept of dependency to emphasize the pivotal influence of international economic conditions and constraints upon the development process.[12] Under conditions of late and dependent develop-

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Lerner, *The Passing of Traditional Society* (New York: Free Press, 1958); Seymour Martin Lipset, "Some Social Requisites of Democracy: Economic Development and Political Legitimacy," *American Political Science Review* 53 (March 1959); Lucien W. Pye and Sidney Verba, eds., *Political Culture and Political Development* (Princeton: Princeton University Press, 1965).

[9] Claudio Véliz, *The Centralist Tradition of Latin America* (Princeton: Princeton University Press, 1980); Howard Wiarda, *Politics and Social Change in Latin America: The Distinct Tradition* (Amherst: University of Massachusetts Press, 1974); C. S. Whitaker Jr., *The Politics of Tradition: Continuity and Change in Northern Nigeria, 1946–1966* (Princeton: Princeton University Press, 1970); Clair Holt, ed., *Culture and Politics in Indonesia* (Ithaca, N.Y.: Cornell University Press, 1972).

[10] David E. Apter, *The Politics of Modernization* (Chicago: University of Chicago Press, 1965); Gabriel Almond and James S. Coleman, eds., *The Politics of the Developing Areas* (Princeton: Princeton University Press, 1960); Daniel Lerner, *The Passing of Traditional Society* (New York: Free Press, 1958).

[11] Examples include Leonard Binder, *Iran: Political Development in a Changing Society* (Berkeley: University of California Press, 1962); Goran Hyden, *Political Development in Rural Tanzania* (Nairobi: East African Publishing House, 1969); T. V. Sathyamurthy, *The Political Development of Tanganyika* (Stanford: Stanford University Press, 1963); Karl Von Vorys, *Political Development in Pakistan* (Princeton: Princeton University Press, 1965); Robert Edward Ward and Ardath W. Burks, *Political Development in Modern Japan* (Princeton: Princeton University Press, 1968); and Robert E. Ward and Dankwart A. Rustow, eds., *Political Modernization in Japan and Turkey* (Princeton: Princeton University Press, 1964).

[12] Seminal works include Fernando Henrique Cardoso and Enzo Faletto, *Dependency and Development in Latin America,* trans. Marjory Mattingly Urquidi (Berkeley: University of California Press, 1979); Andre Gunder Frank, *Capitalism and Underdevelopment in Latin America: Historical Studies of Chile and Brazil* (New York: Monthly Review Press, 1967); Colin Leys, *Underdevelopment in Kenya: The Political Economy of Colonialism* (London: Heinemann, 1974); Theotonio dos Santos, "The Structure of Dependence," *American Economic Review* 60 (May 1970); Osvaldo Sunkel, "National Development Policy and External Dependence in Latin America," *Journal of Development Studies* 6 (October 1969). Related writings include Samir Amin, *Accumulation on a World Scale: A Critique of the Theory of Underdevelopment* (New York: Monthly Review Press, 1974); and Arghiri Emmanuel, *Unequal Exchange: A Study of the Imperialism of Trade* (New York: Monthly Review Press, 1972). For scholarly

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ment, urbanization, social mobilization, industrialization, and other related processes were seen as generating social inequalities and political tensions that spilled over into authoritarianism and political repression. The bureaucratic-authoritarian model of Guillermo O'Donnell, which was originally designed to explain trends in South America and later extended to other world regions, represented an important variant that incorporated not only the delayed and dependent development perspective but also Huntington's partial inversion of modernization theory.[13]

Like Huntington's, both the cultural and structuralist approaches attempted to address the gap separating the optimistic expectations of development theory from contemporary political realities. But whereas cultural theorists emphasized values and historically determined cultural templates, structuralists stressed economic conditions and associated patterns of class formation. To some extent these differences reflected underlying assumptions about the most useful level of social analysis. Those working out of a dependency framework, in particular, presupposed that political development, however defined, could only be understood in terms of the international system as a whole. Other scholars, including those emphasizing the weight of cultural traditions, limited their purview to the national, subnational, or individual level of analysis.

More fundamentally, however, the conflict between structural and cultural approaches reflected divergent and largely unresolvable metatheoretical conceptions about underlying patterns of social causality. Whereas structural frameworks assume that political life necessarily reflects changing economic conditions, cultural approaches to develop-

overviews of the literature, see James A. Caporaso, "Dependency Theory: Continuities and Discontinuities in Development Studies," *International Organization* 34 (Autumn 1980); Cristóbal Kay, *Latin American Theories of Development and Underdevelopment* (London: Routledge, 1989); Philip J. O'Brien, "A Critique of Latin American Theories of Dependency," in Ivan Oxaal et al., eds., *Beyond the Sociology of Development* (London: Routledge and Kegan Paul, 1975); Robert A. Packenham, *The Dependency Movement: Scholarship and Politics in Development Studies* (Cambridge: Harvard University Press, 1992); Gabriel Palma, "Dependency: A Formal Theory of Underdevelopment or a Methodology for the Analysis of Concrete Situations of Underdevelopment?" *World Development* 6 (July–August 1978); Ian Roxborough, "Unity and Diversity in Latin American History, *Journal of Latin American Studies* 16 (May 1984); Tony Smith, "The Underdevelopment of the Development Literature: The Case of Dependency Theory," *World Politics* 31 (January 1979).

[13] Guillermo O'Donnell, *Modernization and Bureaucratic-Authoritarianism: Studies in South American Politics* (Berkeley: Institute of International Studies, University of California, 1973); idem, "Reflections on the Patterns of Change in the Bureaucratic-Authoritarian State," *Latin American Research Review* 13, no. 2 (1978). For an overview and critique of the model, see David Collier, ed., *The New Authoritarianism in Latin America* (Princeton: Princeton University Press, 1979). For extensions of the model to other regions, see Huag Baeg Im, "The Rise of Bureaucratic Authoritarianism in South Korea," *World Politics* 39 (January 1987); William Crowther, "Philippine Authoritarianism and the International Economy," *Comparative Politics* 18 (April 1986).

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ment presuppose that political life is grounded in the realm of ideas and values. To compound these difficulties, the most influential versions of the structuralist account of development processes to appear in the 1970s were penned in the less industrialized countries and cast in the language of Marxism-Leninism, building a political and ideological dimension into the debate. Some of its flavor may be gleaned from the comments of a leading developmentalist, who was led to characterize the dependency literature as an "intellectual-guerrilla movement" representing "a propaganda fragment of an ideology, a polemic against mainstream development theory."[14] On the other side of the divide, a leading *dependentista* bemoaned the distortions and misunderstandings embedded in critiques of the dependency framework, which had set up a debate that he likened to "a dialogue between two deaf people."[15]

Largely unnoticed in this conflict were important areas of theoretical convergence. Not only had the somber realities of the postindependence period generated widespread pessimism regarding the prospects for democratic political development in the Third World, but the prevalence of coercive and authoritarian forms of political domination led scholars working under very different normative and theoretical umbrellas to develop a parallel interest in the state. Particularly in the study of Latin America, scholars drew upon the concept of corporatism to emphasize the contrasts between pluralism and state-dominated patterns of associational life. Reflecting the broader debate over development issues, corporatist outcomes were alternatively seen as a product of a particular cultural tradition or as the outgrowth of a pattern of delayed and dependent development characterized by a weak bourgeoisie, high levels of class conflict, and a strong state.[16] Concomitantly, struc-

[14] Gabriel A. Almond, "The Development of Political Development," in Myron Weiner and Samuel P. Huntington, eds., *Understanding Political Development* (Boston: Little, Brown, 1987), 451, 454.

[15] Fernando Henrique Cardoso, "The Consumption of Dependency Theory in the United States," *Latin American Research Review* 12, no. 3 (1977), 15.

[16] Howard J. Wiarda, *Corporatism and National Development in Latin America* (Boulder, Colo.: Westview Press, 1981); James M. Malloy, ed., *Authoritarianism and Corporatism in Latin America* (Pittsburgh: University of Pittsburgh Press, 1977); Kenneth Paul Erickson, *The Brazilian Corporative State and Working-Class Politics* (Berkeley: University of California Press, 1977); Philippe C. Schmitter, *Interest Conflict and Political Change in Brazil* (Stanford: Stanford University Press, 1971); Alfred Stepan, *The State and Society: Peru in Comparative Perspective* (Princeton: Princeton University Press, 1978); Howard J. Wiarda, "Toward a Framework for the Study of Political Change in the Iberic-Latin Tradition: The Corporative Model," *World Politics* 25 (January 1973); David Chaplin, ed. *Peruvian Nationalism: A Corporatist Revolution* (New Brunswick, N. J.: Transaction Books, 1976); Frederick B. Pike and Thomas Stritch, eds., *The New Corporatism: Social-Political Structures in the Iberian World* (Notre Dame: University of Notre Dame Press, 1974); T. M. Shaw, "Beyond Neocolonialism: Varieties of Corporatism in Africa," *Journal of Modern African Studies* 20 (June 1982).

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turalist accounts of the political experience of Third World countries began to draw upon neo-Marxist theories of the state.[17]

The state thus emerged as a pivotal research focus in each of the two major strands of postdevelopmentalist thinking. Indeed, given the sheer prevalence of titles such as *State and Conflict in the Middle East: The Emergence of the Postcolonial State*, the concept of the state might be said to have achieved the same prominence in the 1980s as modernization in the 1950s, development in the 1960s, and dependency in the 1970s.[18] Notwithstanding the emergence of state-centric understandings of political outcomes, this shift in thematic emphasis did not necessarily imply a fundamental break with established theoretical approaches. For the most part, scholars continued to analyze the state as the manifestation of broader contextual forces. The growth of interest in the state nevertheless marked the beginning of a profound shift away from explanations assigning primacy to micro- or macrolevel variables at the expense of the intermediary or institutional level of societal analysis.

The Resurgence of Political Institutions

By the mid-1980s, the debate between culturalists and structuralists had been overtaken by two related sets of developments—the global processes of democratization and market-oriented reform. Paradoxically, both liberalization processes began in the late 1970s, just as con-

[17] See, in particular, Theda Skocpol, *States and Social Revolutions: A Comparative Analysis of France, Russia, and China* (Cambridge: Cambridge University Press, 1979); Peter B. Evans, Dietrich Rueschemeyer, and Theda Skocpol, eds., *Bringing the State Back In* (Cambridge: Cambridge University Press, 1985); Christian Anglade and Carlos Fortin, eds., *The State and Capital Accumulation in Latin America*, 2 vols. (Pittsburgh: University of Pittsburgh Press, 1990).

[18] Gabriel Ben-Dor, *State and Conflict in the Middle East: The Emergence of the Postcolonial State* (New York: Praeger, 1983). Other relevant examples include Maria Helena Moreira Alves, *State and Opposition in Military Brazil* (Austin: University of Texas Press, 1985); Lisa Anderson, *The State and Social Transformation in Tunisia and Libya, 1830–1980* (Princeton: Princeton University Press, 1986); Douglas C. Bennett and Kenneth E. Sharpe, *Transnational Corporations versus the State: The Political Economy of the Mexican Auto Industry* (Princeton: Princeton University Press, 1985); Thomas M. Callaghy, *The State-Society Struggle: Zaire in Comparative Perspective* (New York: Columbia University Press, 1984); Zaki Ergas, ed., *The African State in Transition* (Basingstoke: Macmillan, 1987); Thomas B. Gold, *State and Society in the Taiwan Miracle* (Armonk, N.Y.: M. E. Sharpe, 1986); Merilee S. Grindle, *State and Countryside: Development Policy and Agrarian Politics in Latin America* (Baltimore: Johns Hopkins University Press, 1986); Nora Hamilton, *The Limits of State Autonomy: Post-Revolutionary Mexico* (Princeton: Princeton University Press, 1982); Joel S. Migdal, *Strong Societies and Weak States: State-Society Relations and State Capabilities in the Third World* (Princeton: Princeton University Press, 1988); Victor Nee and David Mozingo, eds., *State and Society in Contemporary China* (Ithaca, N.Y.: Cornell University Press, 1983); Tim Niblock, ed., *Iraq: The Contemporary State* (New York: St. Martin's Press, 1982); Julius Edo Nyang'oro, *The State and Capitalist Development in Africa: Declining Political Economies* (New York: Praeger, 1989); Donald Rothchild and Naomi Chazan, eds., *The Precarious Balance: The State and Society in Africa* (Boulder, Colo.: Westview Press, 1988); Alain Rouquié, *The Military and the State in Latin America*, trans. Paul E. Sigmund (Berkeley: University of California Press, 1987).

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sensus had been achieved on the undemocratic and statist direction of change in the Third World. Notwithstanding the survival of islands of theoretical continuity, most notably in the study of the Middle East, where scholars are still examining the roots of the authoritarian state,[19] the predictable outcome was theoretical decay. The analytical frameworks that had come to dominate the study of Asian, African, and Latin American politics had been constructed to address the rise of statist and authoritarian modes of governance. None accounted for, much less predicted, the dramatic reversal in global development trends. The comparative field was thus left facing challenging new puzzles with little in the way of theoretical equipment.

In many respects, the condition of the comparative field in the late 1980s resembled that of the late 1960s. New sets of empirical developments had shattered widely shared assumptions about the direction of political change, discredited established approaches, and created room for theoretical and methodological innovation. The challenge confronting comparativists in the 1980s, however, was far more daunting than in the 1960s on three distinct counts. First, as the crisis of authoritarianism and statist economic models achieved global dimensions, the Second and Third Worlds disappeared as conceptual categories, forcing comparativists to think about development issues in far broader geographical terms than in the past. Second, the international dimensions of the transformation process discredited domestically grounded understandings of political life, reduced the theoretical payoff of research conceived within a traditional case-study format, and otherwise called into question the conventions of the comparative subfield. Comparativists were thus challenged to broaden not only their geographical horizons but also their methodological and theoretical ones. Third, and perhaps most important, the breakdown of authoritarian and statist models posed complex sets of questions not readily addressed on the basis of established models or theoretical frameworks, whether cultural or structural in nature.[20]

The pattern of political change in the 1960s had contradicted available theory but still allowed comparativists to rethink the causal relationship between socioeconomic change and democratic stability on the basis of the modernization model. Huntington suggested the pos-

[19] See Jill Crystal, "Authoritarianism and Its Adversaries in the Arab World," *World Politics* 46 (January 1994).

[20] See Karen L. Remmer, "New Theoretical Perspectives on Democratization," *Comparative Politics* 27 (October 1995). See also Paul Starr, "The New Life of the Liberal State: Privatization and the Restructuring of State-Society Relations," in Ezra N. Suleiman and John Waterbury, eds., *The Political Economy of Public Sector Reform and Privatization* (Boulder, Colo.: Westview Press, 1990).

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sibility of a curvilinear relationship between socioeconomic modernization and democratic stability. Dependency theorists went one step farther and posited a negative relationship. While more ambiguous, cultural approaches suggested a relatively flat but positive relationship mediated by traditional culture. In terms of the specification of key variables and direction of causality, however, modernization theory provided a common point of theoretical departure. Politics, whether defined in terms of individual behavior, institutions, or broad sets of regime outcomes, was analyzed as a dependent variable—a reflection of modernization processes, societal values, class structures, or international economic forces, which in turn were defined as exogenous to the political system. The privileged pattern of causality thus ran from society to the polity, rather than the other way around.

In contrast, the recent processes of economic and political liberalization have led comparativists to rethink one-way models linking socioeconomic change with politics and to question the capacity of background variables, whether psychological or socioeconomic in nature, to account for political outcomes. Both of these trends have generated a new interest in political institutions and their economic consequences, taking us back to the April 1965 issue of *World Politics*. Interestingly, that issue of the journal not only included Huntington's contribution to the development debate, which assigned causal significance to the strength and stability of political structures, but also an essay by Mancur Olson Jr. arguing that the relationships between politics and economics should be handled endogenously within a single model. In Olson's words, "When economic development influences political change and political change in turn influences economic development, it is important that the two relationships be considered together."[21]

Hence, far from representing "the vindication of modernization theory,"[22] recent trends toward economic and political liberalization have underlined the cogency of early critiques of the modernization model. What links satellite communications, microchips, or the expansion of literacy with political democracy and capitalist economic development? As emphasized by dependency theorists, not to mention the contemporary tragedy of countries such as Haiti and Zaire, economic advance

[21] Mancur Olson Jr., "Some Social and Political Implications of Economic Development," *World Politics* 17 (April 1965), 527.

[22] Lucien W. Pye, "Political Science and the Crisis of Authoritarianism," *American Political Science Review* 84 (March 1990), 7. According to Pye, "Modernization theory predicted that such developments as economic growth, the spread of science and technology, the acceleration and spread of communications, and the establishment of education systems would all contribute to political change."

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is neither inevitable nor irreversible. Politics and associated patterns of policy choice and implementation matter. Likewise, as argued by Huntington and the many other critics of modernization theory who followed in his wake, there is nothing automatic about the translation of socioeconomic modernization into political democracy or stability. During the 1960s and 1970s, modernization was associated with a deepening pattern of statist authoritarianism. More recently, economic stagnation and decline have led to authoritarian breakdown, paving the way for transitions to democracy and market-oriented policies by societies with widely varying standards of living, levels of industrial development, and educational attainment. The modernization approach, along with the major theoretical alternatives that crystallized during the 1970s, simply leaves analysts standing too far away from the world of political actors and agency to offer insights into these varied longitudinal and cross-sectional patterns of interaction between economics and politics.

The rising interest in political institutions and the economic dimensions of political life that characterizes contemporary comparative research is not rooted in any single intellectual tradition. Some recent work emphasizing state capacities and institutional stability draws its inspiration directly from Huntington's *Political Order in Changing Societies*.[23] Other studies, especially those focused on the political economy of the advanced industrial nations, follow in the footsteps of another classic of the 1960s, Andrew Shonfield's *Modern Capitalism*.[24] Still other research builds upon research on dependency and the state of the 1970s. Yet the discontinuities with the political science literature of the past are at least as marked as the continuities, especially with respect to the growing body of work rooted in neoclassical economics. The "new political economy" (NPE) literature, as it has become known, departs fundamentally from most other strands of postdevelopmentalist thinking in terms of its assumptions about rational actors, the analytical primacy accorded individual self-interest, and its bias against not only the "predatory state," but politics or "rent-seeking behavior" in general.[25]

[23] See, for example, Migdal (fn. 18); Robert Dix, "Democratization and the Institutionalization of Latin American Political Parties," *Comparative Political Studies* 24 (January 1992); Robert W. Jackman, *Power without Force: The Political Capacity of Nation-States* (Ann Arbor: University of Michigan Press, 1993); Scott Mainwaring and Timothy R. Scully, eds., *Building Democratic Institutions: Party Systems in Latin America* (Stanford: Stanford University Press, 1995).

[24] Andrew Shonfield, *Modern Capitalism: The Changing Balance of Public and Private Power* (London: Oxford University Press, 1965). See, for example, John Zysman, *Governments, Markets, and Growth: Financial Systems and the Politics of Industrial Change* (Ithaca, N.Y.: Cornell University Press, 1983).

[25] For an overview of this literature, see Gerald M. Meier, ed., *Politics and Policy Making in Developing Countries: Perspectives on the New Political Economy* (San Francisco: International Center for Growth, 1991).

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Arguably the most proximate source of inspiration for recent comparative research is the policy-oriented literature that emerged in the wake of the oil shocks of the 1970s. As problems of economic adjustment in both developed and less developed nations posed new challenges to Keynesian economic wisdom, scholars began to explore the roots of cross-national variations in economic policy and performance.[26] Why are some countries more successful at adjusting to changing international economic conditions than others? How do variations in societal organization and state institutions influence economic policy outcomes? In the European context, research revolved mainly around the performance contrasts associated with corporatist and noncorporatist patterns of decision making.[27] For the study of the less industrialized nations, the key puzzle involved striking cross-regional differences in development patterns, notably the the economic success of the East Asian states relative to African and Latin American nations. Why had two sets of states, apparently situated so similarly with respect to the international economy, exhibited such disparate patterns of growth and performance?

The puzzle may be highlighted by simple comparisons among nations considered economic leaders in the three major regions of the Third World. Ghana, often cited as "an African success story,"[28] had approximately the same per capita income as South Korea in 1957, when it gained independence ($490 and $491 for Ghana and South Korea, respectively, in 1980 U.S. dollars). Over the next two decades, Korea developed into a major industrial trading power and joined the ranks of the upper-middle-income nations, while Ghana stagnated and slipped into the ranks of the world's least developed states. By 1989 the per capita GDP of South Korea was more than eleven times that of Ghana.[29] A similar contrast in developmental trajectories separates South Korea

[26] For an overview of these trends, see John H. Goldthorpe, ed., *Order and Conflict in Contemporary Capitalism* (Oxford: Clarendon Press, 1984); Ronald Rogowski, "Comparative Politics," in Ada W. Finifter, ed., *Political Science: The State of the Discipline II* (Washington, D.C.: American Political Science Association, 1993).

[27] Seminal studies include Geoffrey Garrett and Peter Lange, "The Politics of Growth," *Journal of Politics* 47 (August 1985); Peter A. Gourevitch, *Politics in Hard Times: Comparative Responses to Economic Crises* (Ithaca, N.Y.: Cornell University Press, 1986); Peter A. Hall, *Governing the Economy: The Politics of State Intervention in Britain and France* (New York: Oxford University Press, 1986); Peter J. Katzenstein, *Corporatism and Change: Austria, Switzerland, and the Politics of Industry* (Ithaca, N.Y.: Cornell University Press, 1984); idem, *Small States in World Markets: Industrial Policy in Europe* (Ithaca, N.Y.: Cornell University Press, 1985); Mancur Olson Jr., *The Rise and Decline of Nations* (New Haven: Yale University Press, 1982); Zysman (fn. 24).

[28] Barbara Stallings, ed., *Global Change, Regional Response: The New International Context of Development* (New York: Cambridge University Press, 1995), 22.

[29] For further elaboration of these contrasts, see Herbert H. Werlin, "Ghana and South Korea: Explaining Development Disparities—An Essay in Honor of Carl Rosberg," *Journal of Asian and African*

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from Chile, a country widely regarded as Latin America's leading example of successful market-oriented reform. Whereas the per capita income of South Korea increased twenty times between 1970 and 1990, Chilean per capita income merely registered a 130 percent increase, without any measurable improvement in social equality.[30]

In attempting to account for such differences, economists have assigned central importance to variations in policy choice, especially to the contrast between the export-oriented industrialization (EOI) strategies of the East Asian nations and the policies of import substitution industrialization (ISI) pursued elsewhere. Such policy contrasts have tended to be overdrawn and to neglect other major sources of variation among regions;[31] nevertheless, they have raised a pivotal question. As posed by Robert Bates, "Why should reasonable men adopt public policies that have harmful consequences for the societies they govern?"[32] It is a question that has moved politics and state institutions back to the center of analysis and spawned a dramatically expanding body of research on political economy—the central research focus of the comparative subfield in the early 1990s.

In contrast to the past, the recent shift in research emphasis has not entailed the elaboration of global concepts, such as development or dependency, which act as surrogates for meaningful theory. Instead, the literature on political economy has been built around a set of themes and research puzzles that have engaged scholars working in divergent analytical traditions and vocabularies. The range and eclecticism of the subfield make it difficult to characterize accurately; nevertheless, research has focused heavily on questions about cross-national variations in economic policy and performance. Why have the industrial development programs of some nations succeeded while others have failed? Why have policy responses to the debt crisis or other rapid sets of changes in the international economy varied across time and space? How do states reform themselves? What explains variations among

Studies 29 (July–October, 1994). It may be noted that in a 1987 essay on political change, Huntington cited the same comparison in support of a cultural interpretation of development outcomes. Samuel P. Huntington, "The Goals of Development," in Weiner and Huntington (fn. 14), 25.

[30] See World Bank, *World Tables 1993* (Washington, D.C.: World Bank, 1993).

[31] See Deborah A. Bräutigam, "What Can We Learn from Taiwan? Political Economy, Industrial Policy, and Adjustment," *Journal of Modern African Studies* 32, no. 1 (1994); Frederic C. Deyo, ed., *The Political Economy of New Asian Industrialism* (Ithaca, N.Y.: Cornell University Press, 1987); Gary Gereffi and Donald L. Wyman, eds., *Manufacturing Miracles: Paths of Industrialization in Latin American and East Asia* (Princeton: Princeton University Press, 1990); Robert Wade, "East Asia's Economic Success: Conflicting Perspectives, Partial Insights, and Shaky Evidence," *World Politics* 44 (January 1992); Gordon White, ed., *Developmental States in East Asia* (London: Macmillan, 1988).

[32] Robert H. Bates, *Markets and States in Tropical Africa: The Political Basis of Agricultural Policies* (Berkeley: University of California Press, 1981), 3.

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newly industrializing nations in the extent of capital flight and short-term financial activity relative to long-term private sector investment? What accounts for the variable success of programs of stabilization and adjustment? Why has the process of market-oriented reform proceeded more smoothly in some countries than in others?

In addressing these and related sets of questions, the burgeoning literature in the field of political economy has drawn upon neo-Marxist, rational choice, and pluralist understandings of institutional forces. For example, in his work on Latin American responses to the debt crisis, Jeffry A. Frieden emphasizes class conflict and the organization of economic interests.[33] Barbara Geddes, revitalizing Huntington's concern with the capacity of public institutions to give substance to public interests, places the self-interest of politicians at the center of her rational choice analysis of state reform.[34] In her book on Mexican political economy, Sylvia Maxfield takes a third, "policy currents" approach and traces variations in investment policy back to the institutional and organizational power of competing policy alliances that cut across state and society.[35] The dominant analytical tendency, however, has been not merely institutionalist but state-centric in nature. The variations in policy orientation and effectiveness explored in recent studies such as *Embedded Autonomy*, *The Politics of Economic Adjustment*, *Pathways from the Periphery*, and *The Political Economy of Policy Reform*,[36] have been repeatedly traced back to the cohesion, autonomy, administrative capacity, and societal linkages of state actors—precisely the same sources of cross-national difference that were emphasized by the contributors to Katzenstein's edited volume, *Between Power and Plenty*, in 1978.[37]

The shift away from societally based explanations for political outcomes also characterizes research concerned with questions of political democratization rather than political economy. Building on process-oriented approaches to the study of democratization as well as upon lit-

[33] Jeffry A. Frieden, *Debt, Development, and Democracy: Modern Political Economy and Latin America, 1965–1985* (Princeton: Princeton University Press, 1991).

[34] Huntington (fn.1), 415; Barbara Geddes, *Politician's Dilemma: Building State Capacity in Latin America* (Berkeley: University of California Press, 1994).

[35] Sylvia Maxfield, *Governing Capital: International Finance and Mexican Politics* (Ithaca, N.Y.: Cornell University Press, 1990).

[36] Peter Evans, *Embedded Autonomy: States and Industrial Transformation* (Princeton: Princeton University Press, 1995); Stephan Haggard and Robert R. Kaufman, eds., *The Politics of Economic Adjustment* (Princeton: Princeton University Press, 1992); Stephan Haggard, *Pathways from the Periphery: The Politics of Growth in Newly Industrializing Countries* (Ithaca, N.Y.: Cornell University Press, 1990); John Williamson, ed., *The Political Economy of Policy Reform* (Washington, D.C.: Institute for International Economics, 1994).

[37] Peter Katzenstein, ed., *Between Power and Plenty: Foreign Economic Policies of Advanced Industrial States* (Madison: University of Wisconsin Press, 1978).

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erature on political institutions in the advanced capitalist nations, scholars have explored the significance of electoral rules, legislative-executive relations, and party systems.[38] The resulting body of research complements recent work on political economy by extending the focus of analysis to political institutions outside the state sector, as well as by seeking to link institutional arrangements with political outcomes other than economic policy and performance. The central theoretical issues addressed by this body of research revolve around the functioning and stability of political democracy.

Four Central Ironies

Much has been gained by the revitalization of interest in political institutions among development scholars. Whereas older approaches to the study of Asian, African, and Latin American politics tended to fragment the comparative field, the rise of institutional analysis has created important areas of theoretical convergence among scholars studying the advanced capitalist states, the former Soviet bloc, and the several regions of the less developed world. Whether guided by rational choice institutionalism, the historical institutionalist tradition, the new institutional economics, or the less consistent or coherent body of work alternatively described as "renewed" or "new institutionalism,"[39] research on all world regions increasingly revolves around the study of the complex interplay of political institutions, preferences, and policy outcomes. As

[38] See, for example, Barry Ames, "Electoral Rules, Constituency Pressures, and Pork Barrel: Bases of Voting in the Brazilian Congress," *Journal of Politics* 57 (May 1995); Ellen Comisso, "Legacies of the Past or New Institutions? The Struggle over Restitution in Hungary," *Comparative Political Studies* 28 (July 1995); Barbara Geddes, "A Comparative Perspective on the Leninist Legacy in Eastern Europe," *Comparative Political Studies* 28 (July 1995); Juan Linz and Arturo Valenzuela, eds., *The Failure of Presidential Democracy* (Baltimore: Johns Hopkins University Press, 1994); Arend Lijphart, ed., *Parliamentary versus Presidential Government* (New York: Oxford University Press, 1992); Arend Lijphart and Carlos H. Waisman, eds., *Institutional Design in New Democracies: Eastern Europe and Latin America* (Boulder, Colo.: Westview, 1996); Matthew Soberg Shugart and John M. Carey, *Presidents and Assemblies: Constitutional Design and Electoral Dynamics* (Cambridge: Cambridge University Press, 1992).

[39] See James G. March and Johan P. Olsen, "The New Institutionalism: Organizational Factors in Political Life," *American Political Science Review* 78 (September 1984); Grant Jordan, "Policy Community Realism *versus* 'New' Institutionalist Ambiguity," *Political Studies* 38 (September 1990); Walter W. Powell and Paul J. DiMaggio, eds., *The New Institutionalism in Organizational Analysis* (Chicago: University of Chicago Press, 1991). Important examples of work carried out within the framework of different institutionalist traditions include Sven Steinmo, Kathleen Thelen, and Frank Longstreth, eds., *Structuring Politics: Historical Institutionalism in Comparative Analysis* (New York: Cambridge University Press, 1992); R. Kent Weaver and Bert A. Rockman, eds., *Do Institutions Matter? Government Capabilities in the United States and Abroad* (Washington, D.C.: Brookings Institution, 1993); Katzenstein (fn. 37); S. D. Krasner, *Defending the National Interest: Raw Materials, Investments and U.S. Foreign Policy* (Princeton: Princeton University Press, 1978); Douglass C. North, *Institutions, Institutional Change, and Economic Performance* (Cambridge: Cambridge University Press, 1990); Margaret Levi, *Of Rule and Revenue* (Berkeley: University of California Press, 1988).

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a result, new bridges have been built across the theoretical chasm traditionally dividing the study of the advanced industrial nations from the rest of the world, thereby enriching the dialogue of the comparative field as a whole and creating avenues for the entry of new theoretical and methodological approaches to the study of Asian, African, and Latin American nations. The rise of rational choice approaches, which have helped to provide plausible microlevel foundations to the study of the political economy of less industrialized nations, is indicative of this trend.[40]

The rise of institutional approaches may also be seen as a reflecting a second key point of convergence in comparative scholarship—the growth of interest in questions of political economy. Whether the geographical focus is the North Atlantic, Eastern Europe, the Middle East, or other less developed regions of the world, political economy represents a central research theme, providing a major impetus to new research on institutions. Standing at the intersection of political inputs and outputs, political institutions represent the "black box" of politics through which societal interests are translated into policies and policy outcomes. Thus as the substantive focus of research has shifted to political economy, institutions have emerged as important sets of explanatory variables. Again the result has been greater theoretical integration of comparative scholarship, as evidenced by the unprecedented growth of cross-regional scholarship and the related increase in the understanding of cross-national variations in the global process of market-oriented reform.[41]

Institutional approaches have made less headway in other important areas of comparative research. The study of democratization, for example, has been dominated by process-oriented analyses emphasizing leadership choice and political contingency.[42] Nevertheless, important theoretical insights have been achieved by placing questions of institutional incentives and constraints at the center of the study of regime

[40] See, for example, Barry Ames, *Political Survival: Politicians and Public Policy in Latin America* (Berkeley: University of California Press, 1987); Larry L. Wade and Sung Jin Kang, "The Democratic Breakout in South Korea: An Informal Game-Theoretic Account," *Asian Perspective* 17 (Fall–Winter 1993); Bates (fn. 32).

[41] See, for example, Haggard (fn. 36); Stephan Haggard and Robert R. Kaufman, *The Political Economy of Democratic Transitions* (Princeton: Princeton University Press, 1995); Tariq Banuri, ed., *Economic Liberalization: No Panacea—The Experiences of Latin America and Asia* (Oxford: Clarendon Press, 1991).

[42] For overviews of the democratization literature, see Daniel H. Levine, "Paradigm Lost: Dependency to Democracy," *World Politics* 40 (April 1988); Karen L. Remmer, "New Wine or Old Bottlenecks? The Study of Latin American Democracy," *Comparative Politics* 23 (July 1991).

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change and consolidation.[43] The strengths of approaches that emphasize the importance of institutional variations are also considerable in terms of suggesting new ways of understanding political democracy. Inter alia, such approaches make it possible to conceptualize political democracy less as the product of a particular set of cultural traditions, historical pattern of development, or level of modernization than as a consensual framework or cooperative equilibrium that may be constructed in a variety of ways, involve varying sets of rules or institutional arrangements, and yield disparate sets of political outcomes.[44] It thereby becomes possible to address the puzzling array of historical, cultural, and societal conditions associated with democracy in the contemporary world without deepening the confusion generated by traditional efforts to theorize about democracy on the basis of comprehensive inventories of variables, explanatory propositions, or causal paths.

The rediscovery of political institutions, however, has also entailed costs, including the reentry of some of the same analytical problems identified by the original critics of development theory. Huntington, like most development and modernization theorists before him, conceptualized the causes and consequences of political change largely in terms of phenomena that were defined at the national level. Polities were more or less stable, more or less democratic, or more or less institutionalized as a consequence of domestic, rather than external, forces. Shifts through time in political stability or regime type were likewise explained mainly in terms of national, rather than international, developments.

The revival of interest in institutions has tended to pull analysts back to the national level of analysis. Variable patterns of linkage with the international system, which were successfully highlighted by more sophisticated versions of dependency theory, are downplayed in favor of the organization of domestic interests and institutions. As emphasized by Barbara Stallings, "Ironically just as international variables became especially important in the 1980s, they disappeared as the key factor from theories of development. As often happens, theory is out of phase with reality."[45]

[43] See, for example, Michael Bratton and Nicolas van de Walle, "Neopatrimonial Regimes and Political Transitions in Africa," *World Politics* 46 (July 1994); Karen L. Remmer, *Military Rule in Latin America* (Boston: Unwin Hyman, 1989); Philip G. Roeder, *Red Sunset: The Failure of Soviet Politics* (Princeton: Princeton University Press, 1993); Baohui Zhang, "Corporatism, Totalitarianism, and Transitions to Democracy," *Comparative Political Studies* 27 (April 1994).

[44] See, in particular, Adam Przeworski, *Democracy and the Market: Political and Economic Reforms in Eastern Europe and Latin America* (New York: Cambridge University Press, 1991).

[45] Barbara Stallings, "International Influence on Economic Policy: Debt, Stabilization, and Structural Reform," in Haggard and Kaufman (fn. 36), 43.

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The second irony is that institutional approaches carving out a relatively autonomous causal role for political processes have risen to the forefront of political analysis precisely when economic constraints have drastically narrowed the range of feasible policy options. By the late 1980s, for example, Latin American governments—whether guided by democratic, socialist, or conservative ideologies, elected on populist or neoliberal platforms, or supported by more or less privileged sets of social actors—were adopting relatively similar programs of market-oriented economic reform. The similarities obviously speak to the importance of economic, not political, causality.

The third irony is that institutional approaches have placed politicians and bureaucrats at the center of analysis just as the activities, resources, and relative weight of the state are being dramatically reduced. At issue is not merely the question of neoliberal reform and attendant processes of "state shrinking," including privatization and deregulation. Also relevant is the expansion of incentives and opportunities for societal organization associated with the global process of political liberalization. Presumably, the scope of public sector decision-making authority has narrowed in response to these changes, leaving a broader array of activities in the hands of societal actors and interests. Likewise, the reduction of political repression, the growth of media independence, the opening of electoral channels of influence, and the growing density of both domestic and transnational civil society arguably constrain the autonomy of political institutions and enhance the importance of societal influences upon political outcomes, including policy formation.

The fourth and final irony is that institutions are being analyzed as independent variables at a time of far-reaching institutional change and instability. Whether one looks at the architecture of the state or the organizational contours of civil society, the institutional infrastructure of societies in Eastern Europe and the former Third World is being rapidly reshaped by the ongoing processes of economic and political liberalization. These trends raise important questions about the causes, rather than the consequences, of political institutions.

The rediscovery of institutions has thus created a gap between theory and empirical reality that rivals the one highlighted by Huntington thirty years ago. Instead of domestically driven processes of policy choice and implementation, political outcomes increasingly reflect the influence of international trade agreements, transnational production systems, multilateral lending institutions, global voluntary associations, and nongovernmental organizations. Instead of state autonomy, the

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globalization of production, capital, information, technology, and even labor markets has strictly delimited the ability of nation-states to set the economic rules within their borders. Instead of state-centric choice processes, there has been a trend toward the reduction in the scope and autonomy of public sector decision-making processes. And instead of institutions establishing relatively stable and predictable frameworks for decision making, the rules of the political game are being widely contested and redefined around the globe. Neither "state-centered" nor broader "polity-centered" institutionalism adequately speak to these trends.[46]

Addressing the ironies of the present without falling into the theoretical pitfalls of the past requires several innovations in the comparative research agenda. First, and perhaps most challenging, comparativists need to shed their resistence to incorporating the international dimension into their explanatory frameworks. Just as economists have found open-economy models useful for addressing contemporary issues of stabilization and adjustment, comparativists need to begin thinking more systematically in terms of "open polity" models. The assumption that international variables represent a constant that can be ignored for the purposes of comparative research is less a product of available theory than of case-study methodology. The relative success of nations in adjusting to the economic challenges of the 1980s, for example, reflected not only the policy preferences and organizational capacity of domestic actors, but varying patterns of international linkage, which created greater opportunities for economic development in Asia than in either Latin America or Africa.[47]

The importance of the international dimension is even more obvious where cross-national similarities are concerned. The literature on regime transitions, for example, has analyzed the breakdown of authoritarianism in Latin America as a series of discrete and unrelated processes of change reflecting disparate, if not unique, patterns of causality. A more plausible line of analysis would begin with changes in the international system and explore the mediating impact of domestic interests and institutions.[48] As Robert Keohane points out, "Analysts focusing on the domestic politics and economics of one or a few countries may ascribe patterns of behavior and outcomes to distinctively na-

[46] I have drawn these terms from Jonus Pontusson's insightful review essay, "From Comparative Public Policy to Political Economy: Putting Political Institutions in Their Place and Taking Interests Seriously," *Comparative Political Studies* 28 (April 1995), 122.

[47] See Stallings (fn. 28).

[48] See, for example, Karen L. Remmer, "The Process of Democratization in Latin America," *Studies in Comparative International Development* 27 (Winter 1992–93).

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tional causes, without recognizing the degree to which common forces affecting a range of countries operate powerfully in each."[49]

The problem is not that approaches emphasizing institutions or other aspects of domestic politics are less than comprehensive or excessively parsimonious. Given the inductive, case-study traditions of the comparative field, the dominant theoretical propensity is arguably just the opposite. The real issue, however, has less to do with parsimony than with the capacity of theories to address the puzzles they are designed to illuminate. Institutional analyses may yield dazzling results;[50] but they are not a panacea that can be called upon to answer any and all sets of research questions, especially if analytical relationships are vaguely specified and pivotal concepts, such as "state capacity" or "embedded autonomy," are not readily operationalized apart from the phenomena they are designed to explain. Such tendencies complicate the larger problem of examining the relative contribution of variables operating at different levels of analysis. For example, if international economic forces represent the obvious theoretical starting point for explaining the global shift to market-oriented policies, then the importance of institutional variables cannot be effectively assessed within the framework of models that only address domestic sources of variation. Likewise, if a nation's capacity to pursue effective development strategies is conditioned by access to external credit and aid, international market opportunities in leading export sectors, "economic neighborhoods," or other similar sets of factors, then state institutions need to be viewed as intervening variables rather than as the appropriate point of theoretical departure.

Although the relevant body of theory is far less developed in the realm of political rather than economic outcomes, the same set of concerns applies. Accounts of democratization that emphasize the role of leaders and contingency offer insights into particular features of a transition process, but they do not begin to address broader patterns of similarity and difference. As suggested by Barbara Geddes's recent essay comparing Latin America and Eastern Europe, the choice of institutional frameworks in new democracies may be understood more effectively in terms of the interests of leaders of new political parties rather than as a product of random or indeterminate choices and

[49] Robert O. Keohane, "The World Political Economy and the Crisis of Embedded Liberalism," in John H. Goldthorpe, ed., *Order and Conflict in Contemporary Capitalism* (Oxford: Clarendon Press, 1984), 15.

[50] See George Tsebelis, "Decision Making in Political Systems: Veto Players in Presidentialism, Paliamentarism, Multicameralism and Multipartyism," *British Journal of Political Science* 25 (July 1995).

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events.[51] Still other questions about democratization may demand broader analytical frameworks that incorporate not only macrosocial but international forces, including transnational institutions, as suggested by the recent research of Alison Brysk and Kathryn Sikkink.[52]

The appropriate level of analysis depends upon the question that is being asked, which is to say that it is the nature of the dependent variable that determines the relative utility of macro-, meso-, or microlevel approaches. Even if the dependent variable is government policy, rather than economic performance or institutional change, the appropriate level of analysis may not be institutional. As emphasized by Jonas Pontusson, "The question of causal significance cannot be treated simply as an empirical problem; any research strategy, and especially one that relies on comparative case studies, presupposes certain theoretical assumptions. Yet such assumptions also cannot be made in the abstract; the question of analytical primacy (or salience) depends on what it is that we want to explain."[53] As suggested above, among the most interesting and important theoretical questions raised by the contemporary context have to do with linkages between international and domestic systems—questions that cannot be adequately addressed on the basis of frameworks that accord causal primacy to domestic political institutions.

The second major challenge that needs to be addressed by comparativists relates to the two-way interaction between politics and economics. In according institutions a privileged analytical position, state- or polity-centered approaches systematically downplay the importance of economic forces. The theoretical costs are high. Efforts to relate effective adjustment efforts or development strategies to state automony,[54] bureaucratic capacities,[55] party-system fragmentation,[56] or other sets of institutional variables, for example, run up against two major sets of an-

[51] Geddes (fn. 38).

[52] Alison Brysk, *The Politics of Human Rights in Argentina* (Stanford: Stanford University Press, 1994); Kathryn Sikkink, "Human Rights Issue-Networks in Latin America," *International Organization* 47 (Summer 1993).

[53] Pontusson (fn. 46), 144.

[54] See, for example, Tony Killick, "Relevance, Meaning and Determinants of Flexibility," in Killick, ed., *The Flexible Economy: Causes and Consequences of the Adaptability of National Economies* (London: Routledge, 1995). He observes: "There is near unanimity that relative state autonomy is one of the factors that has worked to the advantage of the East Asian 'miracle' economies" (p. 4).

[55] See, for example, Joan M. Nelson, *Economic Crisis and Policy Choice: The Politics of Adjustment in the Third World* (Princeton: Princeton University Press, 1990); Stephan Haggard, Richard N. Cooper, and Chung-in Moon, "Policy Reform in Korea," in Robert H. Bates and Anne O. Krueger, eds., *Political and Economic Interactions in Economic Policy Reform* (London: Blackwell, 1993); Louis A. Picard and Michele Garrity, eds., *Policy Reform for Sustainable Development in Africa: The Institutional Imperative* (Boulder, Colo.: Lynne Rienner, 1994).

[56] See, for example, Stephan Haggard and Robert R. Kaufman, "The Political Economy of Inflation and Stabilization in Middle-Income Countries," in Haggard and Kaufman (fn. 36.).

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alytical problems. The first has to do with the direction of causal effects. Party and bureaucratic systems in the less developed regions of the world are in a state of flux, driven forward in many cases by dramatic shifts in economic performance. The collapse of the Apra party and the disintegration of the state bureaucracy in Peru, for example, were a consequence of the disastrous economic legacy of the García administration of the 1985 to 1989 period. Likewise, the success of the Menem administration in Argentina in overcoming hyperinflation between 1989 and 1995 led to major modifications in the Argentine constitution, party system, trade union movement, and policy-making process. In these and many other cases, economic performance has shaped as well as reflected institutional arrangements.

Efforts to analyze the impact of institutional arrangements also need to take into account more compelling theoretical alternatives. The incentives and opportunities to eliminate budgetary deficits, build capable bureaucratic teams, or otherwise tackle problems of stabilization and adjustment reflect economic conditions. Hence, in Latin America, the most dramatic examples of neoliberal reform can be attributed to a military dictatorship (Chile), an uncompetitive one-party regime (Mexico), an aggregative catchall party system (Argentina), and a highly fragmented multiparty system (Bolivia). The list also includes states with high (Chile) as well as rudimentary (Bolivia) bureaucratic capacities. The same diversity characterizes the reluctant and inconsistent reformers, which include aggregative two-party systems (Venezuela) as well as fragmented multiparty systems (Ecuador) and states with reasonably well-developed (Brazil) and primitive (Dominican Republic) bureaucratic capacities. To the extent that organizational and institutional structures have shaped these patterns of similarity and difference, their role is best defined as a complex set of intervening variables whose impact cannot be adequately assessed either on the basis of inductive case studies or within the framework of models that treat economic variables as exogenous.

One by-product of the neglect of economic forces is a tendency toward the compartmentalization of research on political liberalization and economic liberalization.[57] Do market-oriented reforms buttress processes of political liberalization? Does democracy strengthen the hand of economic reformers? The state-centric biases characteristic of institutional approaches to policy formation presuppose that the answer

[57] For a notable exception that explores the complex interplay between economic and political liberalization with specific reference to the transformation of state structures, see Kanishka Jayasuriya, "Political Economy of Democratisation in East Asia," *Asian Perspective* 18 (Fall–Winter 1994).

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to both questions is negative. Yet in Eastern Europe, Latin America, and much of Africa, economic and political liberalization have proceeded hand in hand. The contradiction between theoretical expectations and empirical reality suggests the possible advantages of rethinking the relationship between economic and political change in terms of models that do not assign analytical primacy to state or political institutions.

Similar considerations suggest the utility of integrating institutions more effectively within theoretical frameworks that address the role of societal actors. Even under repressive authoritarian regimes, variations in political outcomes are not just a matter of isolated, top-down, state decision-making processes, but reflect interactions between social forces and the organization of state power.[58] Hence institutional change may result from policy change, rather than vice versa.[59] Likewise, even if the focus of analysis is economic policy, rather than ethnic conflict or other phenomena obviously engaging broad sets of social actors, institutional approaches may not explain significant variations across time and space unless they systematically incorporate societal forces. In bringing institutions back in, comparativists have too often pushed society out, defining the importance of institutions more in terms of their strength and autonomy vis-à-vis society, than in terms of patterns of state-society interaction. As Jeffrey Herbst has emphasized, the importance of institutions has little to do with their strength or weakness: "It is a major mistake to argue that, simply because most African countries lack strong national structures, institutions play an insignificant role in influencing the pattern of resource-allocation politics."[60]

Part of the problem is that much recent research has focused on the processes of policy change, rather than on their causes or consequences, thereby conflating questions of how and why. But, more fundamentally, the statist thrust of much recent institutional research has tended to strip choice processes from their larger political context.[61] For the analysis of recent processes of economic and political liberalization, the result is a pair of disconcerting paradoxes. Drawing upon the insights of neoorthodox economics, the failures of statist developmentalism in the past are being attributed to organized rent-seeking groups in the society at large; the successes of market-oriented reforms in the present, to

[58] See Remmer (fn. 43).

[59] See Bates and Krueger (fn. 55).

[60] Jeffrey Herbst, *State Politics in Zimbabwe* (Berkeley: University of California Press, 1990), 249.

[61] For a cogent critique of this tendency based upon research into automobile industrialization efforts in five countries in East and Southeast Asia, see Richard F. Doner, "Limits of State Strength: Toward an Institutionalist View of Economic Development," *World Politics* 44 (April 1992).

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insulated sets of state actors.[62] The formula is reminiscent of Huntington's managerial perspective on development and is anything but theoretically compelling. The solution to the problem of economic reform is the state, which is also defined as the key obstacle to the operation of markets. What is not explained is why state actors would initiate reforms designed to strengthen markets at the expense of state institutions and, presumably, their own self-interest, much less why elected leaders would choose to resolve the collision between political and economic logics in favor of the latter.

Addressing these issues calls for a broader theoretical perspective. The assumption that market-oriented reforms require insulated decision-making processes or cohesive bureaucratic teams exaggerates the role of state actors at the expense of international economic pressures, political parties, organized interests, and electoral processes. It also underestimates the potential appeal of such reforms not only to business interests but to politicians and electorates. In recent Latin American elections, for example, voters have repeatedly endorsed market-oriented policies over more statist alternatives. The most notable exception is Venezuela, which emphasizes the importance of voters: the electoral defeat of market-oriented incumbents led to a dramatic reversal of neoliberal policies. As with international or economic influences, society cannot be automatically treated as a constant for the purposes of understanding political outcomes. Especially in an era of political and economic liberalization, policy is likely to reflect the diminishing scope of public-sector authority and the expanding organizational and/or economic weight of firms, business associations, political parties, and other sets of social actors.

Downplaying the significance of broader sets of social forces is particularly hazardous if we wish to address the sources of institutional variation across time and space. Why is Africa populated by "soft states?"

[62] According to Haggard and Kaufman, for example, "reform initiatives are more likely where and when political institutions insulate politicians and their technocratic allies from particular interest group constraints, at least in the short run." Haggard and Kaufman (fn. 36), 19. They develop the same argument in *The Political Economy of Democratic Transitions*: "The sucessful initiation of reform depends on rulers who have personal control over economic decision-making, the security to recruit and back a cohesive 'reform team,' and the political authority to override bureaucratic and political opposition to policy change." Haggard and Kaufman (fn. 41), 9. See also Merilee S. Grindle and John W. Thomas, *Public Choices and Policy Change: The Political Economy of Reform in Developing Countries* (Baltimore: Johns Hopkins University Press, 1991); John Waterbury, "The Heart of the Matter? Public Enterprise and the Adjustment Process," in Haggard and Kaufman (fn. 36); Thomas M. Callaghy, "Political Passions and Economic Interests: Economic Reform and Political Structure in Africa," in Thomas M. Callaghy and John Ravenhill, eds., *Hemmed In: Responses to Africa's Decline* (New York: Columbia University Press, 1993); idem, "Lost Between State and Market: The Politics of Economic Adjustment in Ghana, Zambia, and Nigeria," in Nelson (fn. 55).

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What accounts for the institutional capacity of the Asian state? If institutions create winners, as Barbara Geddes has emphasized,[63] why do they change? To the extent that institutions matter, such questions are important and not readily addressed in terms of analyses that accord causal primacy to institutional forces.

These observations lead to a fourth and final suggestion for enhancing our understanding of contemporary development issues, namely, broadening the focus of research to address the sources of institutional change. Research on the advanced industrialized regions of the world can proceed on the assumption that institutions establish relatively stable or at least "sticky" frameworks for political choice and thereby lend predictability to otherwise irrational choice processes. Elsewhere, however, institutional change and instability undercut these assumptions and raise important sets of research questions. The political agenda of the nations of the East and South is crowded with issues of institutional change involving constitutional frameworks, electoral rules, party systems, bureaucratic structures, and interest group organization. The creation of independent central banks (as in Chile), the decline of formerly powerful trade union confederations (as in Argentina), the collapse of established one-party and even two-party systems (as in Hungary and Venezuela, respectively), the expansion of citizenship rights to peasants (as in Mexico), or the privatization of social security, banking, and telecommunications industries (as in Peru) all represent important change in the institutional infrastructure. Such changes shape not only the preferences and power of political actors but also the future viability of ongoing processes of economic and political liberalization. How can we understand the causes and consequences of institutional change? Since institutional approaches characteristically provide better answers for stability than change, the search for answers pushes analysis back to the broader societal and international context.

Conclusion

Development research has undergone fundamental changes over the past three decades in terms of theoretical rigor, methodological sophistication, and the diversification of analytical approaches. Global concepts and frameworks addressing political development, modernization, or stability have all but disappeared in favor of middle-range theories designed to illuminate more specific sets of outcomes. The central

[63] Geddes (fn. 42).

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focus of analytical concern has also shifted from political to economic development, which Huntington and his contemporaries largely took for granted.[64] In the process, phenomena that Huntington identified as problems, such as rapid economic growth, have come to look like solutions. And concentrated government authority, one-party systems, and restricted communications networks, which Huntington saw as solutions, have come to be seen as problems. With the benefit of hindsight, such notions as "The public interest of the Soviet Union is approximated by the institutional interests of the top organs of the Communist Party"[65] seem absurd; the influence of the Communist model over even conservative strands of development theory, astonishing; and the ready dismissal of political freedom and human rights as appropriate development goals for societies now recuperating from brutal dictatorship, callous and misguided. Polities in which public organizations are strong and social forces are weak have long since lost their appeal.

Despite these changes, three decades after the publication of "Political Development and Political Decay," comparativists have joined Huntington in concluding that institutions matter. The consensus is partly attributable to processes of intellectual cycling: institutional approaches emerged as a counterweight or corrective to approaches that exaggerated the explanatory power of social structures or cultural values. Moving away from the micro- and macrolevels of analysis, political scientists converged somewhere in the middle, creating a basis for dialogue among scholars working within the framework of diverse analytical traditions. These trends have been reinforced by the emergence of new research puzzles, especially the effort to understand cross-national variations in economic policy and performance and the rise of democratic rule in unexpected social settings. The utility of institutional approaches for addressing these and related sets of questions has vindicated Huntington's stress upon the importance of institutions in translating macrosocial processes of change into political outcomes.

That institutions matter in this general sense, however, begs most of the critical questions: what institutions matter? and for what? how much do they matter? how and why do they change? Whatever the limitations of his argument, Huntington offered answers for all of these questions. Despite the impressive growth of institutional research, we

[64] See, for example, Joseph LaPalombara, "Bureaucracy and Political Development: Notes, Queries, and Dilemmas," in LaPalombara, ed., *Bureaucracy and Political Development* (Princeton: Princeton University Press, 1963), who, like Huntington, suggested that it would be worth sacrificing speed in economic development to achieve political goals.

[65] Huntington (fn. 1), 413.

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remain far from being able to articulate an equally coherent set of theoretical alternatives. That inability may speak not only to the confusion generated by recent global trends, but to the health of the comparative field, which has abandoned the search for a single approach to the study of institutional phenomena. Yet if the theoretical framework guiding contemporary research amounts to no more than "simply an argument that the organization of political life makes a difference,"[66] the resurgence of interest in political institutions offers little basis for optimism. At issue is not the relevance of political institutions but the development of rigorous, testable, and cumulative thory, which is likely to call for diverse talents and approaches as well as for a more fully developed appreciation of the limitations of institutional analysis. The role of institutions cannot be adequately analyzed within the framework of a research agenda that dismisses the international dimension, ignores intersecting or complementary bodies of theoretical literature, or fails to address the sources of institutional fluidity. The latter issue arguably poses the most profound challenge to institutionalist analyses. As emphasized by the late William H. Riker,

> In the end . . ., institutions are no more than rules and rules are themselves the product of social decisions. Consequently, the rules are also not in equilibrium. One can expect that losers on a series of decisions under a particular set of rules will attempt (often successfully) to change institutions and hence the kind of decisions produced under them. In that sense rules or institutions are just more alternatives in the policy space.[67]

This perspective is particularly relevant to the contemporary world of development, in which global pressures for political and economic liberalization are leading to a rapid redefinition of institutional systems. To the extent that importance is attached to institutions, these processes of change call for more systematic comparative analysis and a related broadening of the institutionalist research agenda.

[66] March and Olsen (fn. 39), 747.

[67] William H. Riker, "Implications from the Disequilibrium of Majority Rule for the Study of Institutions," *American Political Science Review* 74 (June 1980), 444–45.

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THE ECLIPSE OF THE STATE?
Reflections on Stateness in an Era of Globalization

By PETER EVANS*

J. P. NETTL'S classic 1968 article on the state aimed to convince his fellow social scientists that "the thing exists and no amount of conceptual restructuring can dissolve it."[1] Analysis of the state was "not much in vogue," as Nettl put it, and he considered this an intellectual aberration. He was convinced that "stateness"—the institutional centrality of the state—varied in important ways among nations, and that political behavior and institutions could be understood only if the state were brought back into the center of political analysis. The three decades since have thoroughly vindicated Nettl. Issues of stateness regained and retained the kind of centrality that he argued they should have. The debate that he helped revive continues unabated.

While Nettl has been vindicated, the form and content of his vindication are full of ironies. The spread of interest in the state to economics, a discipline almost completely ignored in Nettl's article,[2] has been central to the revival of debate. In part because of this disciplinary shift, the stakes are defined differently. For Nettl, the alternatives to "stateness" were systems of public authority in which other kinds of institutions (parties in Britain, the law and legal institutions in the United States) were salient. Current debates are less about the form of public institutions than about the extent to which private power can (or should) be checked by public authority. Reinvigorated political faith in the efficacy of markets combined with a rediscovery of civil society cre-

*I would like to thank Fred Block, Ha Joon Chang, Neil Fligstein, Stephan Haggard, Atul Kohli, John Ruggie, Theda Skocpol, John Stephens, and especially the members of my graduate seminar in comparative political economy for their useful criticisms of an early draft of this paper and, of course, to absolve them from any responsibility for the directions taken by my response.

[1] Nettl, "The State as a Conceptual Variable," *World Politics* 20 (July 1968), 559.

[2] Nettl was interdisciplinary—his footnotes refer to a wide range of sociologists, political scientists, and historians—but in 1968 he found references to economists or economic logic unnecessary in dealing with debates on the state.

World Politics 50 (October 1997), 62–87

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ates a charismatic set of substitutes for public institutions and a corresponding set of arguments for the "eclipse of the state."

Changing theoretical perspectives cannot be separated from real historical changes in the position of the state. In the brief decades since Nettl wrote, the demands on the state have burgeoned. In the OECD countries demographically driven increases in transfer payments have resulted in a doubling of government expenditures as a proportion of GDP. In developing countries the desire for more rapid economic development produced a similar expansion. Lagging development of political and administrative institutions resulted in an ominous "capacity gap." In some parts of the developing world, most dramatically Africa, real eclipses of the state, in the sense of full-blown institutional collapse, took place. Even where there was no threat of collapse, a worrisome erosion of public institutional capacity seemed to be under way. It was much harder to ignore the state in the 1990s than it was in the 1960s.

Perhaps most ironic, from the perspective of Nettl's analysis, is how changes in the international arena have affected "stateness." For Nettl, the state's role vis-à-vis the international system was "invariant," reinforcing stateness even when domestic institutions denied it.[3] Three decades later the international arena is viewed very differently. The collapse of the old bipolar world has diminished the power of statecentric political and military rivalries to dominate international relations. Simultaneously, the growth of opportunities for transnational economic gains has laid the foundation for a new series of arguments about why states are anachronisms. According to these arguments, the intensified development of economic transactions that cross national boundaries has undermined the power of the state, leaving it marginalized as an economic actor. The arena that Nettl saw as securing stateness is now seen as transcending the power of the nation-state.

Changes in the global ideological climate are as crucial as new flows of money and goods, and Nettl's analysis does anticipate a key aspect of those changes. For Nettl, England was the "stateless society *par excellence*" and "an American sociopolitical self-examination simply leaves no room for any valid notion of the state."[4] Thus, the relative neglect of

[3] Nettl (fn. 1) saw the international arena almost purely in realist terms, arguing that in the international arena the state was "the almost exclusive and acceptable locus of resource mobilization" (p. 563). In Nettl's view, "Here [in the international system] the state is the basic, irreducible unit, equivalent to the individual person in a society" (p. 563). Since the "international function is invariant," "even where the notion of the state is very weak, as in Britain and the United States, the effective extrasocietal or international role is not affected" (p. 564).

[4] Nettl (fn. 1), 562, 561.

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the concept of the state during the twenty-five years preceding his article was a logical consequence of the "shift of the center of gravity of social science to the United States."[5] Today, the untrammeled hegemony of Anglo-American ideological premises is one of the most salient forces shaping the specific character of the current global economy, including the extent to which globalization is viewed as entailing the eclipse of the state.

In this environment pursuing Nettl's agenda requires a different starting point. Statelessness can no longer be treated simply as a feature of Anglo-American political culture. It must be dealt with as a dominant global ideology and potential institutional reality. Therefore, the question of whether the eclipse of the state is likely and, if so, what the consequences of such an institutional shift would be, takes precedence. The trick is to deal with the question of eclipse seriously without taking a positive answer for granted.

I will argue that while eclipse is a possibility, it is not a likely one. What the discourse of eclipse has done is to make responses to a genuine crisis of state capacity unrelentingly negative and defensive. The danger is not that states will end up as marginal institutions but that meaner, more repressive ways of organizing the state's role will be accepted as the only way of avoiding the collapse of public institutions. Preoccupation with eclipse cripples consideration of positive possibilities for working to increase states' capacity so that they can more effectively meet the new demands that confront them. The goal should be to work back toward something closer to Nettl's original agenda of comparing different kinds of "stateness" and their consequences, this time with more explicit attention to the effects of globalization.

I begin by looking at the impact of globalization on stateness and arguing that the structural logic of globalization and the recent history of the global economy can then be read as providing rationales for "high stateness" as well as "low stateness." That I will argue that the absence of a clear logic connecting economic globalization to low stateness makes the normative and ideological side of the global order a key determinant of how globalization affects stateness. I will then move from globalization to a discussion of current theoretical perspectives on stateness, arguing that these perspectives are both sources of insight into the nature of the contemporary global order and influential shapers of the political and ideological face of that order. Finally, I conclude with a discussion of what this analysis implies for future forms of stateness.

[5] Ibid., 561.

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Globalization and the Role of the State

"Diminished," "defective," and "hollow" were typical adjectives applied to the contemporary state in a recent special issue of *Daedalus*.[6] Globalization is not the only reason for the perception that "state authority has leaked away, upwards, sidewards, and downwards" and in some matters "just evaporated,"[7] but it is a central one. The effects of globalization flow through two interconnected but distinct channels. The increasing weight and changing character of transnational economic relations over the course of the last three decades have created a new, more constraining context for state action. The political effect of these structural changes has been channeled by the growing global hegemony of Anglo-American ideology.

The New Global Political Economy

Nettl's concluding assertion that "[t]here remains only the one constant—the invariant development of stateness for each national actor in the international field" has been inverted.[8] Now the presumed invariant is the international arena's negative effect on stateness. As wealth and power are increasingly generated by private transactions that take place across the borders of states rather than within them, it has become harder to sustain the image of states as the preeminent actors at the global level. No one questions that the traditional Waltzian logic of competing "national interests" continues to drive the "interstate system,"[9] but the muted great power struggles of the post–bipolar world leave international relations increasingly contaminated and often overshadowed by the private logic of the global economy. Nettl likened the international arena to a "society" in which states were the "people," but in the current global order the unique political status of states must be balanced against the fact that the most economically empowered "citizens" of the international arena are transnational corporations (TNCs).[10]

The growing relative weight of transactions and organizational connections that cross national boundaries is the cornerstone of globalization. Exports and imports growing more than one and a half times faster than domestic transactions around the world and a doubling of the proportion of exports to GDP in the OECD countries are just the beginning. Foreign direct investment has been growing three times as fast as

[6] *Daedalus* 24 (Spring 1995).
[7] Susan Strange, "The Defective State," *Daedalus* 124 (Spring 1995), 56.
[8] Nettl (fn. 1), 591.
[9] Cf. Kenneth Waltz, *Theory of International Politics* (Reading, Mass.: Addison-Wesley, 1979).
[10] Nettl's formulation is quoted in fn. 3.

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trade, and other sorts of transnational corporate connections (alliances, subcontracting, and so on) have probably been growing even faster.[11]

The impact of both trade and investment is magnified by the changing character of trade. Rather than being an exchange of goods between domestic productive systems, trade is increasingly a flow of goods within production networks that are organized globally rather than nationally.[12] Commodities are created through the integration of production processes performed in a multiplicity of national territories. Whether any given territory is included in global production networks or excluded from them depends on the decisions of private actors. States can try to make their territories attractive, but they cannot dictate the structure of global production networks.

In the classic realist world traditional military forms of statecraft were closely intertwined with possibilities for economic gain. Powerful economic actors were presumed to have an interest in the political and military capacities of "their" states, just as state managers had an interest in the capacities of "their" entrepreneurs. National economic prowess was the foundation of military (and therefore diplomatic) strength. Territorial expansion was a route to control over new productive assets. A world of global production networks makes the prospective economic gain from territorial conquest dubious, reducing the returns to realist statecraft. Access to capital and technology depends on strategic alliances with those who control global production networks, rather than on the control of any particular piece of territory. In a global economy where there is a surplus of labor, control over large amounts of territory and population can be more of a burden than an asset.

As long as private economic actors were dependent on the political environment provided by a particular state, it made sense for them to identify with the political successes and aspirations of that state. National aggrandizement held out the prospect of private profit; threats to sovereignty might also contain threats to profit. The operators of what Robert Reich calls "global webs"[13] have much less reason to identify

[11] Robert Wade, "Globalization and Its Limits: Reports of the Death of the National Economy Are Greatly Exaggerated," in Suzanne Berger and Ronald Dore, eds., *National Diversity and Global Capitalism* (Ithaca, N.Y.: Cornell University Press, 1996). Wade offers a compilation of other such statistics, along with a nicely skeptical and carefully balanced account of the ways in which such statistics may exaggerate globalization.

[12] See Robert B. Reich, *The Work of Nations* (New York: Vintage Books, 1992); Gary Gereffi and Miguel Korzeniewicz, eds., *Commodity Chains and Global Capitalism* (Westport, Conn.: Praeger, 1994). For a comprehensive vision of the consequences of global networks for social organization, see Manuel Castells, *The Information Age: Economy, Society and Culture*, vol. 1, *The Rise of the Network Society* (Oxford: Blackwells, 1996).

[13] Reich (fn. 12).

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with nationalist territorial ambitions and anxieties. From the perspective of these networks, the interstate system as a whole is an essential piece of economic infrastructure and conflicts among states are a source of disruption and uncertainty.

Underlying the transnational mobility of capital and the construction of global production networks is a radically globalized financial system, whose operation poses a fundamental challenge to public authority in the economic realm. There has always been footloose capital and states have often depended on the cooperation of international financiers, but the changes that have taken place in the last two decades are quite extraordinary. When Nettl was writing, the fixed exchange rate system was still in effect and most major industrialized countries continued to exercise controls over capital flows. By the end of the 1980s, by contrast, capital controls had been dismantled and the value of currencies was left more to markets than to states.[14] The effect of the new institutional framework was magnified by advances in communication and information systems.

Vincent Cable offers a succinct summary of the current disproportion between global financial markets and the economic leverage available to individual states: "Foreign exchange trading in the world's financial centers exceeds a trillion dollars a day . . . greater than the total stock of foreign exchange reserves held by all governments."[15] The result is what Fred Block has called "the dictatorship of international financial markets." Any state that engages in policies deemed "unwise" by private financial traders will be punished as the value of its currency declines and its access to capital shrinks.[16]

These processes of globalization certainly contribute to the perceived evaporation of state authority, but the connection is not as straightforward as it might first appear. The state is not eclipsed by the simple fact of its becoming more dependent on trade. Existing cross-national statistics suggest that greater reliance on trade is associated with an increased role for the state rather than a diminished one. Moreover, a

[14] See Fred Block, *The Vampire State and Other Stories* (New York: New Press, 1996).

[15] Cable, "The Diminished Nation-State: A Study in the Loss of Economic Power," *Daedalus* 124 (Spring 1995), 27.

[16] Block (fn. 14). See also Geoffrey Garrett, "Capital Mobility, Trade and the Domestic Politics of Economic Policy," *International Organization* 49, no. 4 (1995). Garrett emphasizes the surprising extent to which European social democracies have been able to resist "the dictatorship of international financial markets," but he leaves no doubt that resistance imposes a growing price. For example, he concludes his study by saying: "[F]inancial markets have imposed significant interest rate premiums on the power of the left and organized labor; and these increased with the removal of barriers to cross-border capital flows. . . . In time, one might speculate that no government would be able to bear this burden" (p. 683).

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look at the nations that have been most economically successful over the last thirty years suggests that high stateness may even be a competitive advantage in a globalized economy.

Twenty years ago David Cameron noticed that the statistical relationship in advanced industrial economies between openness (as measured by the share of trade in GDP) and the size of government was positive rather than negative.[17] The finding suggested a logic as plausible as that connecting globalization and eclipse. Higher trade shares increase a country's vulnerability to externally induced traumas; a larger public sector provides a protective counterweight. Peter Katzenstein's case studies of small European social democracies spelled out the institutional infrastructure underlying the operation of this logic.[18]

These relationships are not simply artifacts of what is now referred to as "the golden age of capitalism" (roughly 1950–73).[19] Recent analysis by Garrett, Kitschelt et al., and others shows how the configuration of public institutions continues to shape the impact of globalization.[20] Dani Rodrik has replicated and extended Cameron's statistical findings using contemporary data. Looking at data on OECD countries for the 1980s and the early 1990s, Rodrik found "a quite strong correlation among the OECD countries between government expenditures (as a share of GDP) and exposure to trade: countries that are more exposed to trade have bigger governments."[21] Furthermore, when he extends the analysis to more than one hundred countries, most of them developing, he not only finds "a striking positive relationship between size of government (in this case government consumption) and exposure to trade" but also finds that "the degree of openness during the early 1960's is a

[17] Cameron, "The Expansion of the Public Economy: A Comparative Analysis," *American Political Science Review* 72, no. 4 (1978).

[18] See Katzenstein, *Small States in World Markets: Industrial Policy in Europe* (Ithaca, N.Y.: Cornell University Press, 1985).

[19] See Paul Bairoch and Richard Kozul-Wright, "Globalisation: Myths and Realities: Some Historical Reflections on Integration, Industrialisation and Growth in the World Economy" (Paper presented at the UNU/WIDER conference on "Transnational Corporations in the Developed, Developing and Transitional Economies: Changing Strategies and Policy Implications," Cambridge University, September 21–23, 1995).

[20] Geoffrey Garrett's analysis (see fn. 16) of data from fifteen OECD countries over the period 1967–90, which included other measures of globalization, helps to further elucidate institutional connections between globalization and the expansion of government. He found that a "coincidence of strong leftist parties, capital mobility, strong trade unions, and high levels of trade led to greater government spending." For another analysis of the ways in which the consequences of globalization are mediated by national institutions, see Herbert Kitschelt et al., eds., *Continuity and Change in Contemporary Capitalism* (New York: Cambridge University Press, forthcoming).

[21] Rodrik, "The Paradoxes of the Successful State" (Alfred Marshall Lecture, delivered during the European Economic Association meetings, Istanbul, August 22–24 [final version, September], 1996a), 31–32. See also idem, "Why Do More Open Economies Have Bigger Governments" (NBER Working Paper no. 5537, April 1996b).

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very good predictor of the *expansion* of government over the subsequent three decades."[22]

A look at contrasting regional growth trajectories over the past thirty years suggests that high stateness may do more than simply insulate domestic populations from external traumas. It may actually be a source of competitive advantage in a globalizing economy. The dizzying growth of transnationally organized production may have been the leading economic headline of the thirty years since Nettl wrote, but the major competing headline has been the spectacular growth of East Asian economies. Few would now dispute that the growth of East Asia over the past fifty years represents a historic shift in the economic hierarchy of nations, one that could eventually prove to be a regional shift comparable to the rise of Northwestern Europe 250 years earlier. If the globalization headline provides grist for the argument that the state is on the wane, the East Asia headline has very different implications for the evolution of stateness.

In the years since Nettl wrote, East Asian states—from Korea in the North to Singapore in the South with the People's Republic of China in the middle—have used various strategies in which the state played a central role to effect dramatic changes in Asia's position in the international division of labor. Obviously the role of the state varies across these cases, but no one would argue that they are stateless societies.[23] They offer a new variety of high stateness, quite different from Nettl's continental European model but perhaps more effective in economic terms.

East Asian successes force us to reexamine the idea that effective participation in a globalized economy is best achieved by restricting state involvement in economic affairs. They suggest that successful participation in global markets may be best achieved through more intense

[22] Rodrik (fn. 21, 1996a), 32.

[23] For a sampling of the now voluminous literature on the role of the state in the East Asian economic miracle, see Yilmaz Akyuz and Charles Gore, "The Investment-Profits Nexus in East Asian Industrialization" (Background paper prepared for the conference on "East Asian Development: Lessons for a New Global Environment," Kuala Lumpur, Malaysia, February 29 and March 1, 1996). See also the published version, *World Development* 24, no. 3 (1996); Alice Amsden, *Asia's Next Giant: South Korea and Late Industrialization* (New York: Oxford University Press, 1989); José Edgardo Campos and Hilton L. Root, *The Key to the Asian Miracle: Making Shared Growth Credible* (Washington, D.C.: Brookings Institution, 1996); Tun-jen Cheng, "The Politics of Industrial Transformation: The Case of the East Asia NICs" (Ph.D. diss., University of California, Berkeley, 1987); Peter Evans, *Embedded Autonomy: States and Industrial Transformation* (Princeton: Princeton University Press, 1995); Stephan Haggard, *Pathways from the Periphery: The Politics of Growth in Newly Industrializing Countries* (Ithaca, N.Y.: Cornell University Press, 1990); Chalmers Johnson, *MITI and the Japanese Miracle: The Growth of Industrial Policy, 1925–1975* (Stanford, Calif.: Stanford University Press, 1982); Robert Wade, *Governing the Market: Economic Theory and the Role of Government in Taiwan's Industrialization* (Princeton: Princeton University Press, 1990); World Bank (IBRD), *The East Asian Miracle: Economic Growth and Public Policy* (*A World Bank Policy Research Report*) (New York: Oxford University Press, 1993).

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state involvement. Singapore is the most obvious case in point.[24] Singapore is not only a highly internationalized economy in terms of its extreme reliance on trade, but it is also exceptionally dependent for its local economic dynamism on foreign direct investment by transnational corporations. At the same time it is equally renowned for the capacity and power of its state bureaucracy.

This case, anomalous as it may be in terms of the conventional wisdom, underscores what should be logically obvious: small countries bargaining with large TNCs may do better if a competent, unified national agenda participates in the bargaining on the local side. There are many ways to make profits, some of them quite consistent with rising wages and high rates of local reinvestment. If a state can credibly promise an infrastructure consistent with such strategies, along with a predictable set of rules and competent rule makers with whom to dialogue, it is hardly surprising that there is no dearth of TNCs disposed to join the game.[25]

East Asia demonstrates the possibility of a positive connection between high stateness (albeit not Nettl's classic European variety) and success in a globalizing economy and puts historical meat on Cameron and Rodrik's regression results. If such a positive connection exists, then the currently pervasive belief that the institutional centrality of the state is incompatible with globalization must be explained in terms of the ideological face of the current global order.

Ideology and Interests in the Global Order

In any international regime, norms, formal rules, and shared assumptions are as important in shaping the role of the state as the flows of goods and capital. John Ruggie made the point impeccably fifteen years ago in his explication of how the global political economy of the golden age came to be characterized by "embedded liberalism."[26] Liberalism, in the sense of relatively unrestricted freedom for global capital, was

[24] On Singapore's internationalized strategy of development and the role of the state bureaucracy in this strategy, see Campos and Root (fn. 23); Cheng (fn. 23); Gillian Koh, "A Sociological Analysis of the Singapore Administrative Elite: The Bureaucracy in an Evolving Developmentalist State" (Ph.D. diss., University of Sheffield, England, 1995); Jonathan Quah, "The Public Bureaucracy and National Development in Singapore," in K. K. Tummala, ed., *Administrative Systems Abroad* (Washington, D.C.: University Press of America, 1982); idem, "The Rediscovery of the Market and Public Administration: Some Lessons from the Singapore Experience," *Australian Journal of Public Administration* 51, no. 3 (1993); Hilton L. Root, *Small Countries, Big Lessons: Governance and the Rise of East Asia* (Hong Kong: Oxford University Press, 1996).

[25] See Peter Evans, "TNCs and Third World States: From the Old Internationalization to the New," in Richard Kozul-Wright and Robert Rowthorne, eds., *Transnational Corporations and the Global Economy* (London: MacMillan, forthcoming).

[26] Ruggie, "International Regimes, Transactions and Change: Embedded Liberalism in the Postwar Economic Order," *International Organization* 36 (Spring 1982).

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"embedded" in a social compact that committed the advanced industrial states to insulating (at least partially) their citizens from the costs of such a system. Embedded liberalism was also an Anglo-American construction, but it was the product of an Anglo-American ideology significantly constrained by post–World War II fears that failure to protect domestic populations might reinitiate the political traumas of the preceding decades.

Like embedded liberalism, the current regime is a means of uniting the contradictory principals of national sovereignty (the keystone of the interstate system) and economic liberalism (which presumes that states will restrain their desire to exercise sovereignty over economic transactions that cross their borders). What is distinctive about the current regime is first of all the degree to which economic gain can be pursued independently of sovereignty and, second, the hegemony of a version of Anglo-American ideological precepts remarkably untrammeled by anxieties over potential political instability. Finally, unlike embedded liberalism, which was conceived of primarily as a regime for the industrialized West, the current normative regime is presumed to apply to rich and poor alike.

Whether active state involvement can increase the benefits that a country's citizens garner from the global economy becomes a moot point in an ideological climate that proscribes using territorial sovereignty to limit the discretion of private economic actors. In the current global order Anglo-American ideological prescriptions have been transcribed into formal rules of the game, to which individual states must commit themselves or risk becoming economic pariahs. GATT and the WTO are only the most obvious formal manifestations of the doctrine that as far as capital and goods are concerned the less individual states behave as economic actors, the better off the world will be.[27] Bilateral negotiations, at least those to which the United States is a party, convey the message even more aggressively.[28] The private representatives of international financial capital and, in the case of developing countries, international financial organizations like the IMF, impart the same tutelage.

[27] For a general discussion of changes in regulatory fashion since the golden age, see Ha-Joon Chang, "The Evolution of Perspectives on Regulation in the Postwar Era," Working Paper (Washington, D.C.: Economic Development Institute of the World Bank, 1995). For a discussion of the way in which the WTO constrains economically nationalist strategies such as industrial policy, see V. R. Panchamuki, "WTO and Industrial Policies," Study no. 7, UNCTAD Project on East Asian Development: Lessons for a New Global Environment (Geneva: United Nations, 1996).

[28] See, for example, Peter Evans, "Declining Hegemony and Assertive Industrialization: U.S.-Brazilian Conflict in the Computer Industry," *International Organization* 43 (Spring 1989).

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The effect of global ideological consensus (sometimes aptly labeled the "Washington consensus") on individual states goes well beyond the constraints imposed by any structural logic of the international economy. The fact that becoming more actively engaged in trying to improve local economic conditions risks the opprobrium, not just of powerful private actors, but also of the global hegemon makes any state intervention a very risky proposition. An ideology that considers such action neither possible nor desirable does, however, at least release the local state from responsibility for whatever economic woes its citizenry may suffer at the hands of the global economy. Even richer states, with more highly developed institutional capacities for insulating their populations from economic uncertainty, are under the same pressure. They are more likely to resist and indeed have done so,[29] but given the asymmetries of international power, it is hard for any individual state to shift the balance.

The current order fits the ideological proclivities of both the only remaining superpower and the private firms that dominate the global economy. The question is whether it speaks effectively to their interests. If an economically stateless world could deliver in practice a global equilibrium that met the needs of TNCs, then eclipse might indeed be in the offing. In fact, transnational investors trying to integrate operations across a shifting variety of national contexts need competent, predictable public sector counterparts even more than do old-fashioned domestic investors who can concentrate their time and energy on building relations with a particular individual government apparatus.[30]

The same argument applies even more strongly to global financial capital. The "dictatorship of international finance" is really closer to a mutual hostage situation. The operation of the international financial system would descend quickly into chaos without responsible fiscal and monetary policies on the part of international actors. Financial markets can easily punish deviant states, but in the long run their returns depend on the existence of an interstate system in which the principal national economies are under the control of competent and "responsible" state actors. Those who sit astride the international financial system need capable regulators. The lightning speed at which transactions of great magnitude can be completed makes for great allocational effi-

[29] Cf. Garrett (fn. 16); Kitschelt et al. (fn. 20).

[30] For a general discussion of the extent to which firms rely on states to create and sustain markets, see Neil Fligstein, "Markets, Politics and Globalization" (Manuscript, Berkeley, 1996). See also idem, "Markets as Politics: A Political-Cultural Approach to Market Institutions" *American Sociological Review* 6 (August 1996).

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ciency in theory, but it also makes for great volatility in practice. "Rogue traders" are (as the name implies) supposed to be aberrations; yet the possibility of enormous returns from speculative activity makes the rogue role a continual temptation.[31] After a certain point, reducing the power of states to interfere increases collective exposure to risk more than it expands the possibilities for individual profits.

The fact that private transnational actors need competent, capable states more than their own ideology admits does not eliminate the possibility of eclipse. The calculations of even sophisticated managers are biased by their own worldviews. Bent on maximizing its room for maneuver, transnational capital could easily become an accomplice in the destruction of the infrastructure of public institutions on which its profits depend. Up to a point, constricting the ability of states to intervene in global markets may produce increased profits. By the time state capacity is so reduced that the unpredictability of the business environment becomes intolerable, even to major actors who have wide latitude in choosing where to do business, reconstructing public authority could be a long and painful process, even an impossible one.

The complicated interactions that connect the global order and domestic politics make miscalculation more likely. Accepting the prevailing global ideology constrains the ability of governments to protect ordinary citizens, especially those who bear the costs of shifts in the configuration of international production networks. Whether it is the Bolivian state cutting domestic expenditures for health and education in order to remain in conformity with the latest restructuring plan or the Clinton administration pushing through NAFTA in order to demonstrate its full faith in the free international movement of goods and capital, the perception of those who lack privileged positions vis-à-vis international markets is likely to be the same. The state is perceived, not as the ultimate representative of national interests, but instead as the instrument of dimly understood but somehow "foreign" interests.[32] Should transnational managers decide that it were in their interest to foster the reconstruction of state capacity, they would have to overcome accumulated political alienation, as well as reverse institutional atrophy.

If eclipse does occur, it will not be the inexorable result of any ironclad structural logic. The economic logic of globalization does not in it-

[31] See Block (fn. 14).

[32] In the Third World there is, of course, a long-standing tradition of seeing the state in these terms, that is, as a "tool of imperialism." In the United States a lively folk tradition is rapidly developing along analogous lines. As nonsensical as fears of "black helicopters" and visions of the U.S. government as a pawn of the UN may be, this folk mythology does reflect an underlying sense that U.S. administrations are more responsive to transnational actors than to domestic pressure from below.

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self dictate eclipse. While globalization does make it harder for states to exercise economic initiative, it also increases both the potential returns from effective state action and the costs of incompetence. Only when viewed through the peculiar prism of our current global ideological order does globalization logically entail movement toward statelessness. This global ideological order grows, in turn, as much out of the prejudices and ideologies of dominant global actors as out of any logic of interests. Given the degree to which political effects of global economic change are mediated by superimposed interpretative frames, contemporary theoretical perspectives on the state become consequential, not just for the insights they offer, but also because of their potential impact on policy.

New Perspectives on the State

Nettl helped spark a continuing, many-stranded debate on the nature and role of the state. Some of the strands consisted of efforts to demonstrate why variations in stateness must be a central element in political and economic analysis. They revitalized and refined pre-Nettl perspectives, like those of Weber, Hintze, and Gerschenkron, and added new arguments to them.[33] Other strands jibed better with the normative and ideological side of the emerging global order. The flourishing of neoclassical political economy and the renewed fascination with civil society are two of the best examples. The logic of each is quite independent of arguments about globalization, yet both resonated well with a global order built on Anglo-American visions of statelessness. These politically successful formulations must, however, be considered together with less public salient counterposing arguments that raise new reasons for the continuing importance of stateness. Once this is done, the weight of new perspectives on the state lies as much on the side of persistent stateness as on the side of eclipse.

New Economic Perspectives

Of the many strands of thinking on the state that have emerged in the thirty years since Nettl wrote, none has been more thoroughly incorporated into the public political debate than the "neoutilitarian"[34] version of neoclassical political economy. While this line of reasoning was quite

[33] For a review of early efforts in this direction, see Peter Evans, Dietrich Reuschemeyer, and Theda Skocpol, eds., *Bringing the State Back In* (New York: Cambridge University Press, 1985), especially the initial essay by Skocpol.

[34] See discussion in Evans (fn. 23), chap. 2.

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independent of arguments for the historical inevitability of eclipse based on the supposed exigencies of globalization, it reinforced them, suggesting that eclipse might be not only inevitable but also desirable.

During the "golden age" most economists were willing to treat the state as a black box. Economics was a source of prescriptions for policies that would best promote economic growth, but it was not prominent as a tool for the institutional analysis of the state itself. As capitalist growth began to look more problematic in the mid-1970s, this changed. Ironically, the weak performance of market economies in the 1970s and 1980s in those countries where state involvement was least extensive (that is, Britain and the United States) was advanced as evidence of excessive public power over the economy.[35]

Concern with optimizing state policies continued but it was joined by efforts to analyze the institutional mechanisms that underlay "bad" policies (that is, those that did not jibe with economic prescriptions). Analysts of "rent seeking" conceptualized policy-making as an exchange: in return for political and material support, state bureaucrats produced rules that enabled private economic actors to reap unproductive rents.[36] States expanded, not because of increased demand for collective goods, but because of self-seeking bureaucrats. Rent seeking took what had been traditionally seen as aberrant, corrupt practices and transformed them into the core of the political economy of public institutions. In this framework approaches like Nettl's, in which the establishment and the maintenance of norms were preeminent among the state's outputs, did not make sense.

Reconceptualizing the state as a vehicle for rent seeking made it much easier to characterize state intervention as intrinsically pathological. Older arguments about the inefficiencies of bureaucracy and the impossibility of gathering sufficient information to make good policy were trumped by this reinvigorated neoclassical political economy. If the negative effects of state policies were a logical consequence of the nature of public institutions, then better information, more competent officials, and more knowledgeable advisers were not remedies. The only rational strategies for alleviating the problem were then either reducing the resources allocated by these perverse institutions to an absolute

[35] Cf. Linda Weiss and John M. Hobson. *States and Economic Development: A Comparative Historical Analysis* (Cambridge, England: Polity Press, 1995).

[36] See James M. Buchanan, Robert D. Tollison, and Gordon Tullock, eds., *Toward a Theory of Rent-Seeking Society* (College Station: Texas A&M University Press, 1980); David Collander, ed., *Neoclassical Political Economy: An Analysis of Rent-Seeking and DUP Activities* (Cambridge, Mass.: Ballinger, 1984); Anne O. Krueger, "The Political Economy of the Rent-Seeking Society," *American Economic Review* 64 (June 1974).

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minimum or somehow "marketizing" the administrative structure itself, replacing reliance on norms of public service with the hard constraints of marketlike incentive systems.

Keynesian arguments in favor of state intervention were rejected as outmoded. Using taxes to channel some of society's collective output into public endeavors was equated with "the old practice of bleeding a patient with leeches in order to make the patient healthy."[37] The grudging acceptance of using the state as a means of ameliorating the lot of those disadvantaged by market outcomes, which had prevailed during the golden age, was supplanted by the firm conviction that as surely as private greed produced public good through the market, public welfare efforts only served to stunt the economic virtues of its recipients. Private economic power, constrained as little as possible by the distorting hand of public policy, was once again touted as the best protector of the public good, and the ideology of statelessness took on a harder, more aggressive edge.

Neoutilitarian models did provide an elegant way of explaining the corruption and venality that are undeniable facets of most public bureaucracies. As explanations of such pathologies, perspectives that focus on rent seeking are very useful. If, however, they crowd out all other interpretations of public behavior, leaving public authority a synonym for rent seeking and venality, they run the danger of becoming a self-fulfilling prophecy. To the extent that the prevalence of neoutilitarian views strips a career in public service of prestige and legitimates removing the resources public agencies need to deliver real services to constituents, rent seeking becomes indeed the only reasonable motivation for joining the public sector. Once norms and traditions of public service have been destroyed, reinstituting them on a piecemeal basis is an overwhelming task.

While neoclassical political economy was easily assimilated into policy analysis, other innovations, with more fundamental implications for economic theories of production and exchange, were harder to incorporate. The "new growth theory," which provided more elegant ways of formally endogenizing technological change and brought the idea of increasing returns back into the center of economic debates,[38] could easily be read as legitimating an expanded role for the state,[39] but was

[37] Speech by Senator Kyl in the Senate, January 20, 1995, quoted in Fred Block (fn. 14).

[38] For a sophisticated but nontechnical exposition, see Paul Romer "The Origins of Endogenous Growth," *Journal of Economic Perspectives* 8 (Winter 1994).

[39] For example, in the view of Garrett (fn. 16), "'New Growth' theory contends that active government involvement in the economy (for example, public spending on education, physical infrastructure, and research and development) may actually increase productivity and hence competitiveness by

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nonetheless extremely difficult to translate into policy prescriptions. Admitting the possibility of increasing returns also required accepting the fact that the evolution of markets and competition was often highly path dependent[40] and consequently characterized by multiple equilibria. This in turn made it harder to argue that unfettered markets could be counted on to automatically maximize efficiency (or welfare) but did not necessarily point to strategies that could improve on market outcomes.

The consequences of this vision of economic growth are magnified by the fact that an increasing number of products—from software to media images—are more ideas than things. Since the cost of reproducing an idea is essentially zero, returns increase indefinitely with the scope of the market. In an economy of "ideas" subject to increasing returns rather than "things" subject to decreasing ones, the distribution of income and profits is especially dependent on appropriability. The magnitude of returns to an idea does not flow from a logic of marginal production costs in a meaningful sense of the term, but it does depend on authoritative decisions, like the determination of the duration of copyright and patent protection and the intellectual property regime more generally.

As an economy produces more ideas, authoritative enforcement of property rights becomes both more difficult and more critical to profitability. In a global economy this requires an active, competent state that is able to secure the compliance of other states with its rules. In short, the most privileged economic actors in a global information economy (that is, global companies like Disney or Microsoft whose assets take the form of ideas) do not need weaker states; they need stronger ones, or at least states that are more sophisticated and active enforcers than the traditional "night watchman state."

The growing centrality of struggles over appropriability is evident from the global economic policies of the United States over the course of the last two decades. From "super 301s" to GATT negotiations to threatened cancellation of China's most-favored-nation status because of software piracy, the question of intellectual property rights has become a key facet of U.S. international economic policy. While other

providing collective goods that are undersupplied by the market" (p. 658). Others, like Paul Krugman, would argue that government efforts to exploit the theoretical possibilities revealed by the new growth theory are likely to do more harm than good; see, for example, Krugman, *Peddling Prosperity: Economic Sense and Nonsense in the Age of Diminished Expectations* (New York: W. W. Norton, 1995). Nonetheless, even Krugman would not deny that new theoretical possibilities have been opened up.

[40] See, for example, Brian W. Arthur, "Positive Feedbacks in the Economy," *Scientific American* (February 1990).

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forms of regulation are in disrepute, this particular kind of policing is now treated as one of the cornerstones of economic civilization.

Intellectual property rights are a specific instance of a general point. In the complex exchange of novel intangibles, authoritative normative structures, which are provided in large measure by the state, become the keystones of efficient exchange. The new institutional economics, with its emphasis on the necessity of governance structures and the pervasive importance of institutional frameworks to any kind of economic transactions, further generalizes the argument that efficient markets can exist only in the context of effective and robust nonmarket institutions.[41]

Neoclassical political economy offers a good rationale for eclipse, but a broader look at the evolution of economic theorizing reinforces the conclusions that flowed from our earlier examination of globalization itself. Powerful transnational economic actors may have an interest in limiting the state's ability to constrain their own activities but they also depend on a capable state to protect their returns, especially those from intangible assets. In this optic, the persistence of the state's institutional centrality looks more likely than eclipse.

Civil Society and the State

The conviction that allocational efficiency can be achieved independently of public control creates only a partial ideological frame at best. Neither idealized visions of individuals interrelating by means of bilateral voluntary exchanges nor the actual experience of capitalist markets relieves basic anxieties about the maintenance of public order or assuages nostalgia for the satisfactions of traditional community ties. New perspectives on governance that highlighted the potential role of civil society provided a nice complement to the economic side of Anglo-American ideology.[42] Even though they were not global perspectives in themselves, they fit with the new global order. The political triumph of the stateless Anglo-American world order, as reflected in the implosion of state-socialist societies, was an important impetus to the charisma of civil society. The revitalization of civil society was por-

[41] See, for example, Douglass C. North, *Institutions, Institutional Change and Economic Performance* (Cambridge: Cambridge University Press, 1990); and Oliver Williamson, *The Economic Institutions of Capitalism* (New York: Free Press, 1985).

[42] For a sampling of the variety of ways which civil society has captured the imagination of social scientists, see Larry Diamond, "Rethinking Civil Society: Toward Democratic Consolidation," *Journal of Democracy* 5 (July 1994); Ernest Gellner, *Conditions of Liberty: Civil Society and Its Rivals* (New York: Penguin Press, 1994); John A. Hall, ed., *Civil Society: Theory, History and Comparisons* (London: Polity Press, 1995); Paul Wapner, "Politics beyond the State: Environment Activism and World Civic Politics," *World Politics* 47 (April 1995).

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trayed, at least by conservatives, as a solution to the social and political side of public well-being, one that could make the state politically obsolete, just as global markets made the state economically obsolete.

To fit with the prevailing global order, a focus on civil society had to repress the possibility that social and political malaise might flow from the pervasive marketization of social relations instead of from the overbearing intrusion of the state. This, of course, was precisely what conservative interpreters did. Rather than seeing the unchallenged dominion of market relations as the primary obstacle to a revitalization of traditional community ties, they saw the intrusive efforts of the state, ostensibly aimed at enhancing welfare, as "crowding out" community.[43] Just as neoclassical political economy negated the state's role in the development of a more productive and efficient society, the growing charisma of civil society (and other more parochial and exclusionary forms of community) negated the state's ability to speak to nonmarket needs.[44]

In many cases, the vision of an engaged, organized citizenry unseating oppressive state elites fit the historical facts. The oppositional movements that helped bring down moribund state-socialist apparatuses in Eastern Europe were a prime example. The process of replacing authoritarian military regimes with electoral democracies in Latin America produced a similar surge of civil society as a counterweight to the leviathan of state power. Even in these cases, however, the idea that civil society could provide a substitute for the organized public institutions of the state proved unrealistically optimistic. In both Eastern Europe and Latin America, the reinvigoration of civil society proved harder to sustain once the unifying focus of opposition to authoritarian rule was dissipated by political success. Even these cases called for a more complex theory of state-society relations.

Here again, as in the case of economic theorizing, a closer look at contemporary thinking on civil society reveals much that is at odds with the global order's assumption that fostering civil society requires accelerating the eclipse of the state. The proposition of a zero-sum relation between the robustness of state institutions and the vibrancy of civil society is contested even by some of those most convinced of the indispensability of civic associations. A growing body of work suggests

[43] James Coleman, *Foundations of Social Theory* (Cambridge: Belknap Press of Harvard University Press, 1990), 321.

[44] Civil society was, of course, the most universalistic basis for organizing communities. Exclusionary ethnic and religious categories represented potent alternative bases for redefining the nation and reconstituting the footing of public authority. While Nettl did not anticipate the upsurge of neoclassical political economy, he did see (fn. 1) the "snapping of the link between state and nation" (p. 560) as a reason for projecting a general decline in stateness, especially in the developing world.

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that relations between the state and civil society are more productively thought of in terms of mutual empowerment or synergy.

Robert Putnam's brief polemic with Joel Migdal provides a good starting point. Based on his reading of the relationship between "social capital" and the efficacy of regional governments in Italy, Putnam takes issue with Migdal's work, which seems to suggest that strong societies result in weak states and that one of the necessary conditions for the emergence of strong states is a "massive societal dislocation, which severely weakens social control."[45] Not so, argues Putnam, "civic associations are powerfully associated with effective public institutions . . . strong society, strong state."[46] What Putnam's perspective suggests is that just as modern markets depend on economic decisions being nested in a predictable institutional framework, likewise civic engagement flourishes more easily among private citizens and organized groups when they have a competent public sector as an interlocutor.

Others have drawn the same lesson from very different contexts. Looking at Africa, the region in which the disintegration of both state organizations and civil society has been most dramatic, Naomi Chazan argues for a "symbiotic relationship between state and civil society."[47] During the crisis of the 1970s and 1980s "both state agencies and social networks experienced a process of implosion."[48] Conversely, where there has been recovery from the crisis, the "reemergence of intermediate social groups" has "come together with the definition and reassertion of state capacities, highlighting the close connection between civil society and stateness."[49] Vivienne Shue, looking at what might be con-

[45] Migdal, *Strong Societies and Weak States: State-Society Relations and State Capabilities in the Third World* (Princeton: Princeton University Press, 1988), 269.

[46] Putnam, *Making Democracy Work: Civic Traditions in Modern Italy* (Princeton: Princeton University Press, 1993), 176. In part, the differences between Putnam and Migdal result from their different definitions of a strong society. Migdal focuses on vertical, clientelistic ties and parochial relationships based on primordial affinities like ethnicity and kinship. Putnam focuses on civic associations that foster ties among social equals and that, while they may be deeply rooted in history, are modern rather than primordial in form. Putnam's version of society is, however, the one that is relevant to the vision that the emergence of civil society will permit the withering away of formal leviathans of repressive public authority. What optimistic proponents of civil society have in mind when they work toward fostering its rebirth in Eastern Europe or its reinvigoration in Latin America is presumably neither the strengthening of clientelistic ties nor the reawakening of primordial loyalties and parochial prejudices but rather the kinds of horizontal civic associations that are the focus of Putnam's argument. I am indebted to Patrick Heller for drawing this point to my attention; cf. Heller, "Social Mobilization and Democratization: Comparative Lessons from Kerala" (Paper presented at the annual meetings of the Association for Asian Studies, April 1996).

[47] Chazan, "Engaging the State: Associational Life in Sub-Saharan Africa," in Joel Migdal, Atul Kohli, and Vivienne Shue, eds., *State Power and Social Forces: Domination and Transformation* (Cambridge: Cambridge University Press, 1994), 258.

[48] Ibid., 269.

[49] Ibid., 278.

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sidered the polar opposite case—the People's Republic of China—finds a similar kind of mutual empowerment of state and society. The vicissitudes of state-society relations under communist and postcommunist rule demonstrate, she says, "an intriguing relationship . . . between the emergence of a robust sphere of civil associational life, on the one hand, and the consolidation of social power in a relatively strong or resilient state organization, on the other."[50]

Examining what he calls "the browning of Latin America" over the course of the 1980s, Guillermo O'Donnell sets out the converse argument in a way that is consonant with the mutual empowerment hypothesis of Chazan and Shue.[51] First, he points out that "current attempts at reducing the size and deficits of the state as bureaucracy . . . are also destroying the state-as-law and the ideological legitimation of the state."[52] He then argues that the crisis of the state leads to a degeneration of civil society in which community organization and civic engagement are replaced by an "angry atomization."[53]

These broad arguments that the fate of civil society is linked to the ability of the public sector to sustain itself have interesting counterparts at the microlevel. Studies scattered throughout the developing world find evidence for "state-society synergy."[54] Effective development projects at the microlevel often involve state agencies working in combination with local social groups. The possibility of coproduction, in which state agencies and local communities work together to produce a needed service or collective good, is associated in turn with state apparatuses that have sufficient esprit de corps and bureaucratic sophistication to move beyond mechanistically imposing the simplest possible set of centralized rules. The archetypal case in point is Taiwan's irrigation associations, which are built around a subtle melding of centralized bureaucratic decision making and real involvement of local villagers, including substantial community control over the process of local water allocation.[55] States with weaker bureaucracies are incapable of sustain-

[50] See Shue, "State Power and Social Organization in China" in Migdal, Kohli, and Shue (fn. 47), 66.

[51] O'Donnell, "On the State, Democratization and Some Conceptual Problems: A Latin American View with Glances at Some Postcommunist Countries," *World Development* 21, no. 8 (1993). O'Donnell uses the term "browning" to refer to "territorial evaporation of the public dimension of the state" (p. 1358), that is, the spread of areas in which both effective bureaucracies and "properly sanctioned legality" (p. 1359) are lacking.

[52] O'Donnell (fn. 51), 1358.

[53] Ibid., 1365.

[54] See articles by Burawoy, Evans, Fox, Heller, Lam, and Ostrom in the special section on "Government Action, Social Capital and Development," *World Development* 24 (June 1996); and also Judith Tendler, *Good Government in the Tropics* (Baltimore: Johns Hopkins University Press, 1997).

[55] See the article by Lam (fn. 54). See also Michael P. Moore, "The Fruits and Fallacies of Neoliberalism: The Case of Irrigation," *World Development* 17, no. 11 (1989).

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ing the kind of locally oriented bureaucratic competence that makes co-production possible.[56]

The evidence for positive-sum relations between the effectiveness of civic associations and state capacity is not limited to the Third World. Even in the stateless United States, there are interesting historical examples of state-society synergy. In examining the founding of the Children's Bureau, one of the most successful early developments in the U.S. welfare state, Skocpol shows how a broad spectrum of geographically dispersed women's voluntary associations were critical to this "statebuilding for women and children."[57] She also notes the parallels between this "maternalist" effort and the relationship between voluntary associations of farmers and the U.S. Department of Agriculture, another example of state-society synergy generating widespread social and economic change.[58]

If this work is right, a sustained efflorescence of civil society may well depend on the simultaneous construction of robust, competent organizational counterparts within the state. Conversely, a state-society synergy view implies that a move toward less capable and involved states will make it more difficult for civic associations to achieve their goals and will thereby diminish incentives for civic engagement. In the most extreme case, the result would be a globalized version of O'Donnell's "browning."[59] Once again, a closer examination of recent theoretical perspectives, political this time rather than economic, suggests an interest, not in eclipse, but in the maintenance or expansion of state capacity. In this case, however, it is the interests of ordinary members of civil society that are at play rather than those of transnational elites.

The Future of Stateness

Newer political perspectives, like those in economics, contain as many arguments in favor of strengthening state capacities as in favor of eclipse. Analysis of these perspectives produces results that parallel those drawn from the analysis of globalization itself. Together, these arguments lead us to expect states to play a persistent role in the future of

[56] See Ostrom's (fn. 54) discussion of Nigeria.

[57] Theda Skocpol, *Protecting Soldiers and Mothers: The Political Origins of Social Policy in the United States* (Cambridge: Harvard University Press, 1992), 480–91.

[58] Ibid., 486. See also Theda Skocpol and Kenneth Finegold, "State Capacity and Economic Intervention in the Early New Deal," *Political Science Quarterly* 97 (Summer 1982). For a recent reprise of Skocpol's perspective, see Skocpol, "Unravelling from Above," *American Prospect* 25 (March–April 1996).

[59] O'Donnell (fn. 51).

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the global political economy, but such an outcome can hardly be taken for granted. Just because an information-oriented, globalized system of production would seem to depend even more than prior economies on the competent exercise of public authority does not mean that the institutional bases of such authority will survive. Just because the historical experience of those countries most successful at adapting to the modern globalized economy has been characterized by high levels of state involvement does not mean that their experience will be reflected in the institutional arrangements that prevail globally. Most obvious of all, a positive association between a more vibrant civil society and more capable state institutions will not prevent both from disappearing.

What then can be said about the future prospects of stateness in this era of globalization? One reasonable and optimistic hypothesis looks to the return of the ideological pendulum. In this view, the recent push to reduce the role of the state represented a natural reaction to the previous overreaching of politicians and state managers. The glaring capacity gap led to a period during which, in Dani Rodrik's words, "excessive optimism about what the state would be able to accomplish was replaced by excessive pessimism." Rodrik suggests further that, having now surmounted "excessive pessimism," we are "on the threshold of a serious reconsideration of the role of the state in development, one that will lead to an improved understanding of the role that governments can (and have to) play."[60]

This perspective makes sense. States took on more than they could handle during the period following World War II. Dealing with the capacity gap clearly required rethinking the state's role. Readjustment was necessary, and overzealousness in reducing the state's role, natural. The return of the pendulum need not sanction a return to the past, but it would legitimate new efforts to turn states into effective instruments for the achievement of collective goals. The question is whether the pendulum is likely to come to rest at a point that reflects dispassionate analysis of accumulated global experience with regard to the relative effectiveness of different forms and strategies of state action.

Nettl, however, brings a historical and ideological dimension to the story that makes the efficient middle seem harder to attain. His argument that "an American sociopolitical self-examination simply leaves no room for any valid notion of the state" suggests a hegemon unlikely to assess the proper position of the pendulum dispassionately.[61] To this

[60] Rodrik (fn. 21, 1996a), 2–3.
[61] Nettl (fn. 1), 561.

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must be added the problem that the nonstate actors most powerful in defining the global normative order are private corporate elites whose view of where the pendulum belongs is colored by their irreducible interest in protecting private managerial prerogatives. Generating a set of global norms that will encourage the search for ways to reduce demands on public institutions but also support the necessary enhancement of state capacity will require substantial ideological revisionism.

Grounds for expecting such a shift are scanty, but there are some. The shifting balance of economic dynamism in the interstate system is a possible source of revisionism. So far, the implications of East Asia's extraordinary economic success for the kind of stateness that is most effective in a globalized economy have found surprisingly little place in official discourse. Official (as opposed to academic) analysis has been remarkably obdurate in its suppression of the revisionist implications of East Asia's experience.[62] Nonetheless, the eventual assimilation of distinctively East Asian experiences of stateness into global discourse on the state seems inevitable.[63] Any movement in this direction would attenuate the current bias toward eclipse.

The increasing reluctance of the United States to shoulder, as hegemon, the burden of delivering what others perceive to be valuable global collective goods may also serve as an impetus for ideological change. Susan Strange, for example, argues that current asymmetries of interstate power have created a situation in which "the most powerful are able to block, even veto, any exercise of authority in global issues of the environment, of financial regulation, or of the universal provision of basic needs for food, shelter and health care."[64] She then presses further, saying that "the only way to remove the present hegemonic, do-nothing veto on better global governance is to build, bit by bit, a compelling opposition based on European-Japanese cooperation but embracing Latin Americans, Asians and Africans who share some of the same interests and concerns for the future."[65] However remote the prospects for this kind of collective action, Strange's argument does identify another potential impetus for ideological shift.[66]

[62] See, for example, Robert Wade, "Japan, the World Bank and the Art of Paradigm Maintenance," *New Left Review* 217 (May–June 1996).

[63] Some would argue that the East Asian experience already constitutes a competing model, at least for the developing countries of the region itself. See Barbara Stallings and Wolfgang Streeck, "Capitalisms in Conflict? The United States, Europe and Japan in the Post–Cold War World," in Stallings, ed., *Global Challenge, Regional Response: The New International Context of Development* (New York: Cambridge University Press, 1995).

[64] Strange (fn. 7), 71.

[65] Ibid.

[66] Another potential source of normative change—also unlikely but still intriguing—is to be found in the networks of public organizations and officials that are part of the global order. John Meyer, in a

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These relatively implausible fonts of ideological change are likely to have consequences only insofar as they help to further undercut the already ambivalent relation of transnational business to any project of eclipse. Much as TNCs value the elimination of the ability of states to restrict their managerial prerogatives, they also recognize the benefits of dealing with robust and capable public interlocutors. They have an interest in keeping state managers on the defensive—something that escalating rhetoric on the inevitability and desirability of eclipse accomplishes nicely. But they also have an interest in avoiding the real institutional marginalization of the state. This above all else makes a return of the pendulum likely.

The main problem with a return-of-the-pendulum perspective is that it is easily conflated with a return to "embedded liberalism." Even if a return of the pendulum is more likely than eclipse, the threat of eclipse still shapes stateness. What the current global ideological environment does is to ensure that responses to a genuine crisis of capacity will be defensive. Strategies aimed at increasing state capacity in order to meet rising demand for collective goods and social protection look foolish in an ideological climate that resolutely denies the state's potential contribution to the general welfare. Beleaguered state managers and political leaders, bent on trying to preserve the state as an institution (and their own positions), may come up with some innovative organizational improvements and some salutary ways of reducing the scope of what states attempt, but their primary strategy is likely to be reneging on the old commitment to embedded liberalism. The problem of closing the capacity gap is redefined as a project of constructing a leaner, meaner kind of stateness.

In the most sinister version of this leaner, meaner stateness, politicians and state managers gain support for the state as an institution in return for restricting the state's role to activities essential for sustaining the profitability of transnational markets. The capacity to deliver services that the affluent can supply privately for themselves (for example, health and education) is sacrificed, while the more restricted institutional capacity necessary to deliver essential business services and secu-

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classic article, presents a strong case for the collective power of public officials to shape global norms at the transnational level; see Meyer, "The World Polity and the Authority of the Nation-State," in Albert Bergesen, ed., *Studies in the Modern World System* (New York: Academic Press, 1980). Meyer's general model is unconvincing, especially in view of more recent changes in global ideology, but there are some very interesting, if modest, examples of transnational networks rooted in public institutions that have effected change in the global normative order. See, for example, Peter Haas, "Banning Chlorofluorocarbons: Epistemic Community Efforts to Protect Stratospheric Ozone," *International Organization* 46 (Winter 1992).

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rity (domestic and global) is maintained. In turn, delivering security means devoting more resources to the repression of the more desperate and reckless among the excluded, both domestic and international.

Rescuing embedded liberalism would require a very different configuration of state-society relations and a correspondingly different kind of stateness, one founded on relations of mutual empowerment between state institutions and a broadly organized civil society of the kind suggested by Chazan and her colleagues.[67] Engaging the energy and imagination of citizens and communities in the coproduction of services is a way of enhancing the state's ability to deliver services without having to demand more scarce material resources from society. The increased social approbation that comes with more effective, responsive service then becomes an important intangible reward for those who work within the state. Since such a strategy simultaneously rewards the reinvigoration of civil society, thereby augmenting the reservoir of potential participants in coproduction, it is almost certainly subject to increasing returns. Like the returns to the development of bureaucratic forms of organization in an earlier era, the returns to more innovative forms of stateness based on state-society synergy could be prodigious.

Unfortunately, the movement toward eclipse has already made this kind of institutional development unlikely. The kind of capacity necessary to make the state a dependable partner in a strategy of state-society synergy is already in scarce supply. Civic groups are correspondingly less likely to be attracted to strategies of mutual empowerment that involve state agencies. Legitimate disillusionment with the state's capacity to deliver, exacerbated by the pervasive antistate discourse of the Anglo-American global order, has solidified into a domestic political climate that makes engaging the state as an ally seem farfetched. Finally, and perhaps most important, private elites are likely to see a political threat in any form of state-society synergy that involves subordinate groups.

The political prospects of state-society synergy are slim, but they should not be discounted altogether. For beleaguered state managers or politicians disenchanted with leaner, meaner stateness, the political attractions of a strategy of state-society synergy are obvious. It promises a way out of the currently asphyxiating capacity gap. It also promises to generate a set of allies potentially much less ambivalent about the value of public institutions than are the business elites who constitute the principal political pillar of the leaner, meaner state. The logic is equally

[67] See Migdal, Kohli, and Shue (fn. 47), especially Kohli and Shue, "State Power and the Social Forces: On Political Contention and Accommodation in the Third World."

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powerful from the point of view of civic organizations. Leaner, meaner will do little for them. They need capable state organizations to put their policy preferences into practice even more than TNCs need states to guarantee the global business climate. Leaner, meaner is still more likely, but the possibility that state apparatuses might forge new alliances with civic actors in the early decades of the new millennium is no less implausible than the alliances that were actually forged between labor organizations and the state during the early decades of the twentieth century.

Probing beneath the rhetoric of globalization and eclipse reveals a problematic quite consistent with Nettl's original admonitions. Projecting the institutional evaporation of the state provides little more illumination than ignoring it altogether. Preoccupation with eclipse distracts attention from serious ongoing shifts in the nature of stateness. It also inhibits exploration of more promising forms of stateness. Becoming mesmerized by the power of globalized production and exchange is equally counterproductive. Whether the future unfolds in the probable direction of a leaner, meaner state or embodies more unlikely elements of state-society synergy does not depend on the economic logic of globalization alone. It also depends on how people think about stateness.

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LIJPHART, LAKATOS, AND CONSOCIATIONALISM

By IAN S. LUSTICK*

WHEN political scientists seek validation for their research practices, many now cite Imre Lakatos, the late Jewish/Hungarian/British mathematician/epistemologist. Although Lakatos had an explicit animus against social science, his epistemology, methodological strategies, and terminology have proved enormously attractive to its practitioners, especially political scientists.[1] Particularly notable is the regular invocation of Lakatos's notion of competing "research programs"—streams of studies, experiments, and projects based on sets of rules telling scientists what questions to ask ("the positive heuristic") and what questions not to ask ("the negative heuristic").[2]

Lakatos's theory of how science can progress includes both a positivist analysis of how evidence and inference can be used to build better theory and a consideration of the self-interested and quintessentially political mobilization of resources and colleagues. The tension between these two partners in science—evidence and politics—is never completely resolved in Lakatos. In his influential essay, presented in 1965 and first published in 1970, Lakatos propounded a view of science as influ-

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[1] For Lakatos's harsh views on the behavioral sciences, see Lakatos, "Falsification and the Methodology of Scientific Research Programmes," in Imre Lakatos and Alan Musgrave, eds., *Criticism and the Growth of Knowledge* (Cambridge: Cambridge University Press, 1970), 176n. One finds additional evidence of the growing influence of Lakatos's work among political scientists in the prominent methodological guide by three political scientists—Gary King, Robert O. Keohane, and Sidney Verba—who invoke Lakatos as something of an epistemological authority for their approach. See King, Keohane, and Verba, *Designing Social Inquiry: Scientific Inference in Qualitative Research* (Princeton: Princeton University Press, 1994), 12. See also the Winter–Spring 1995 issue of *Critical Review*, containing a wide-ranging symposium on the criticisms launched against rational choice theory by Donald P. Green and Ian Shapiro, in *Pathologies of Rational Choice Theory: A Critique of Applications in Political Science* (New Haven: Yale University Press, 1994). At least six of the contributions, both pro and con, based themselves in part on their interpretations of Lakatos's theory of research programs.

[2] Lakatos (fn. 1), 132. Most references by social scientists to Lakatos's work are to this essay. Between 1980 and 1995 the *Social Science Citation Index* lists an annual average of 10.5 inches of citations to works by Lakatos.

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enced by processes of inference, evidence, and theory testing, combined with the political skills of entrepreneurial scientists whose grantsmanship, intellect, reputations, and rhetorical talents could sustain "degenerative" research programs or fail to advance "progressive" ones.[3] As Alan Musgrave has explained, "early" Lakatos explained the success or failure of research programs as based on the degree of corroboration achieved for important claims and the extent to which "new empirical content" was produced by theories refined by testing. According to Musgrave, however, Lakatos shifted in some of his later writings toward a more political explanation for patterns of success and failure of research programs—expecting that the outcome among competing programs would be determined by the relative abilities of scientist-protagonists to mobilize economic, reputational, and institutional resources, both inside and outside the academy. Accordingly, "late" Lakatos (as Musgrave put it) was unwilling to tell a scientist to stop working on a research project because of explanatory or predictive failure.[4]

Almond and Lijphart: Competing Research Programs in an Early-Lakatosian Mode

The differences between early- and late-Lakatosian views on how research programs progress or degenerate, prevail or disappear, reflect a fundamental dispute regarding the balance between evidence and politics in contemporary social science. Few research programs in comparative politics have concentrated as much sustained effort on as distinct an array of questions as that centered on how democracy can stably operate in culturally plural, fragmented, or deeply divided societies. The trajectory of the consociational research program, as inaugurated by Arend Lijphart's 1969 article in *World Politics*, "Consociational Democracy,"[5] therefore presents a valuable opportunity to explore this balance in Lakatos and in political science.

[3] In his later work Lakatos specifically emphasized those portions of his 1965 paper, published in expanded form in Lakatos (fn. 1), that portrayed the progress of science in "instrumental" terms. See Lakatos, "Replies to Critics," in *Boston Studies in the Philosophy of Science* 8 (1971), 182n.

[4] Alan Musgrave, "Method or Madness?" in R. S. Cohen et al., eds., *Essays in Memory of Imre Lakatos* (Dordrecht, Netherlands: D. Reidel, 1976). In this same volume Paul Feyerabend makes a very strong case that in 1965 Lakatos had already come to this conclusion but that it was camouflaged in his essay "Falsification and the Methodology of Scientific Research Programmes" by his own preferences that science be otherwise than it is. See Feyerabend, "On the Critique of Scientific Reason." Lakatos's discomfort with this skepticist interpretation of his work, as well as his inability to deny its validity, is well reflected in his posthumous published review of Stephen Toulmin's book *Human Understanding*. See "Understanding Toulmin," in *Mathematics, Science, and Epistemology: Philosophical Papers of Imre Lakatos*, vol. 2, ed. John Worrall and Gregory Currie (Cambridge: Cambridge University Press, 1978).

[5] Arend Lijphart, "Consociational Democracy," *World Politics* 21 (January 1969).

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Of course, Lakatos himself cannot be expected to guide our judgment of the substantive merits of consociational theory. In late-Lakatosian terms the very duration of the discussion of consociationalism and the foundation it laid for Lijphart's spectacular scholarly career would signal its outstanding success.[6] Whether the consociational research program can or ever did meet the criteria for good science advanced by early Lakatos is another question.

Indeed, I will show that the success of the consociational research program cannot be explained on the basis of its explanatory power or heuristic value—the criteria for good science advocated in early Lakatos. Rather, its success bears witness to the late-Lakatosian claim that research programs can succeed by relying on the political and rhetorical skills of their leading practitioners and on alliances between those practitioners and political interests outside the scientific arena. The paper proceeds with an examination of the evolution of the central claims of consociational theorists, especially Lijphart, in response to criticism and a changing evidentiary base.

In his 1968 book, *The Politics of Accommodation*, Lijphart cited classics of political philosophy (Aristotle, Mill, Rousseau, Tocqueville, and Madison) and well-known pluralist, group, and conflict theorists (including Leslie Lipson, Seymour Martin Lipset, Arthur F. Bentley, David B. Truman, and Lewis Coser) to portray the Dutch case as "deviant" with respect to what he described as three propositions of "pluralist theory."[7] These were

1. "that viable democratic government faces grave obstacles in . . . societies with clearly discernible racial, linguistic, and religious differences"[8]
2. that "the existence of many secondary groups . . . help(s) preserve moderation and individual freedom"[9]
3. (quoting Lipset) "that the chances for stable democracy are enhanced to the extent that groups and individuals have a number of crosscutting, politically relevant affiliations"[10]

Lijphart also related his discussion to Gabriel Almond's 1956 contention that instability was to be expected in culturally heterogeneous

[6] See the discussion below on Lijphart's most recent intervention in this discussion; and see Arend Lijphart, "The Puzzle of Indian Democracy: A Consociational Interpretation," *American Political Science Review* 90 (June 1996).

[7] Arend Lijphart, *The Politics of Accommodation: Pluralism and Democracy in the Netherlands* (Berkeley: University of California Press, 1968), 2.

[8] Ibid., 3.

[9] Ibid., 5.

[10] Ibid., 7.

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societies—those divided by "mutually reinforcing cleavages."[11] But Lijphart's treatment of Almond in the book was perfunctory; nor did he use the terms "consociationalism" or "consociational democracy." Instead, he employed the phrase "the politics of accommodation"—an evocative formulation, suggestive of Holland's distinctive democratic success. He did not, however, translate that into a model of "accommodative" or "consociational" democracy that could produce stable democracy within a heterogeneous society—a framework that would require a substantial change in Almond's typology.

Such a model is precisely what Lijphart offered in his 1969 *World Politics* article. He described it as a "research note," intended as "a constructive attempt to refine and elaborate Almond's typology of democracies"—a typology anchored in the expectation of democratic stability in socially integrated societies and of instability in fragmented societies.[12] Lijphart argued that there was need for a new category in Almond's typology of democracies—the heterogeneous society/stable democracy. His logic, captured in the idea of "consociationalism," was rendered plausible by the stable operation of democracy in such "culturally fragmented" or "divided" societies as Belgium, the Netherlands, Switzerland, and Austria.

Although some of its formulations were eventually rejected by Lijphart himself, "consociationalism" as a model of political behavior and institutions had its clearest articulation in the 1969 article, which achieved some prominence by virtue of its publication in *World Politics*. It also served as the theoretical basis of a widely read 1974 volume on the topic edited by Kenneth D. McRae.[13] Hence, its characterization of consociational democracy and the conditions under which it could be expected to flourish largely defined the notion of "consociational democracy" for the wider field of comparative politics.[14]

The article emphasizes the key role leaders of rival cultural groups can play. These elites can understand the explosive dangers of majori-

[11] Ibid., 9–10.

[12] Lijphart (fn. 5), 207.

[13] McRae, ed., *Consociational Democracy: Political Accommodation in Segmented Societies* (Toronto: McClelland and Stewart, Carelton Library no. 79, 1974).

[14] Referenced either directly from *World Politics* or from the McRae volume, the article was listed as cited by the *Social Science Citation Index* at least 120 times between 1970 and 1994. An earlier article—Lijphart, "Typologies of Democratic Systems," published in the very first issue *Comparative Political Studies* 1 (April 1968)—was listed as cited approximately seventy times during the same period. Although Lijphart referred to it in his *World Politics* article in the following year as "earlier and briefer" (p. 207n), the *Comparative Political Studies* article was not only longer but also in some ways a more systematic presentation of his basic ideas. For my purposes, it is sufficient to note that the analytic posture Lijphart adopted in that article was even more markedly positivist and methodologically orthodox

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tarian democracy in societies where "political culture is deeply fragmented" or where society is "divided by sharp cleavages."[15] Such leaders can then build institutions and foster policies to stabilize democracy by constraining certain forms of democratic competition. Lijphart takes pains to point out that such a possibility is ignored by Almond.

In setting out his challenge to Almond, Lijphart cites five publications by the latter that drew on pluralist theory to distinguish between three types of modern societies where democracy had been attempted. These were democracies of the "Anglo-American" type; "multiparty democracies," exemplified by the Scandinavian countries, the Low Countries, and Switzerland; and "immobilist democracies," whose prototypes were postwar Italian democracy, the Third and Fourth Republics in France, and Weimar Germany. Almond explained democratic stability in the first category, the "Anglo-American" type (including the "Old Commonwealth democracies"), by stressing crosscutting alliances associated with individuals linked by multiple group affiliations. According to this analysis, the "homogeneous, secular political culture" of these systems, combined with a unified but highly differentiated role structure, produces cross-pressures that militate against stigmatizing cultural groups. Potentially stigmatized groups manage to escape that fate because they contain individuals who are, or are likely to be, political allies of potential stigmatizers. As a result members of any population segment are discouraged from identifying more strongly with their own segment than with the society as a whole. Hence, even winner-take-all majoritarianism can produce stability, since the number of decisions/elections is not finite and the composition of majorities is expected to change significantly. Today's losers, in other words, can support the system and acquiesce in decisions they oppose out of a belief that they will be tomorrow's winners.

According to Lijphart, Almond attributed instability in the "continental European democracies" (the immobilist type) to the relative absence of "the kinds of 'cross-pressures' that moderate [their] rigid political attitudes." Almond thus explained the immobilism of these systems as being "a consequence of the [fragmented] condition of the political culture."[16] According to both Almond and Lijphart, democracy in such societies is paralyzed by cultural fragmentation and can rather easily give way to instability, authoritarianism, or even totalitarianism.

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than his approach in the *World Politics* version—a posture that, as we shall see, he substantially abandoned in the process of defending his ideas.

[15] Lijphart (fn. 5), 208.

[16] Ibid., 209, 208.

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The second category, including Scandinavia, the Low Countries, and Switzerland, appears in only some of the studies by Almond cited by Lijphart. Almond refers to these countries, according to Lijphart, as representing merely a combination of "some of the features of the Continental European and the Anglo-American" political systems—of standing "somewhere in between the Continental pattern and the Anglo-American."[17] Lijphart, by contrast, believes these states constitute a category of stable democracy with its own distinctive logic. He then takes Almond to task for imprecisions and inconsistencies, for failing to theorize the pattern of stability they exhibit, and for failing to investigate or even notice the anomaly represented by cases of democratic stability that occur in the absence of the conditions specified as essential by pluralist theory.

Lijphart then asked a penetrating and conceptually sophisticated question. "In what respects are Switzerland, Scandinavia, and the Low Countries 'in between' the Anglo-American and Continental European democracies?" Rejecting Almond's suggestion of a continuum of variation in democratic stability as linked to homogeneity of political culture, extent of role structure integration, or the aggregative function of political parties, Lijphart judged Almond's typology fundamentally unsound. The small European democracies, Lijphart argued, did not fall in the middle of this continuum—neither with respect to the amount of democratic stability they display, nor with respect to the factors Almond presents as explaining variation in democratic stability.[18] Rather, Lijphart concluded, the evidence called for abandoning the idea of a single continuum in favor of a typology with a qualitatively distinct category of democracies, one that achieved and maintained stability by utilizing divisions among cultural segments.

Lijphart at work in the late 1960s was meticulous in his conceptual and evidentiary dissection of Almond's work. Operating as a committed positivist social scientist, Lijphart was ready to accept (and exploit) Popperian and early-Lakatosian norms of scientific rigor, conceptual and definitional clarity, theoretical explicitness, and cruel use of evidence in the evaluation, refinement, and development of theories about political life. Lijphart treated Almond fairly, that is, as a researcher attempting to extend the range of claims about democratic stability to a wider comparative context while adhering to the core precepts and assumptions of the pluralist research program. Accordingly, Lijphart could use the standards of precision, consistency, and evidence within

[17] Ibid., 207.
[18] Ibid., 210–11.

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that program to identify the limitations of its core precepts. Finding that the pluralist research program could not remain true to the hard core of its presuppositions, Lijphart launched his own research program on how elite choice and "consociationally" structured political institutions could produce democratic stability—without cultural homogeneity, unified, society-wide systems of role differentiation, and pervasive crosscutting cleavages.

Lijphart explained democratic stability in his "deviant cases" by identifying a variable ignored by the pluralist research program.

> According to the theory of crosscutting cleavages, one would expect the Low Countries, Switzerland, and Austria, with subcultures divided from each other by mutually reinforcing cleavages, to exhibit great immobilism and instability. But they do not. . . . [A] third variable can account for the stability of the consociational democracies: the behavior of political elites.[19]

Lijphart argued that despite tension, instability, and competition among elites, segment leaders can also produce stability, by making "*deliberate efforts to counteract the immobilizing and unstabilizing effects of cultural fragmentation.*"[20] In contrast to alternative interpretations of consociationalism, Lijphart's view of these efforts was *not* as separately undertaken, serendipitously complementary policies. Rather he described, as the "essential characteristic of consociational democracy, not so much any particular institutional arrangement as the deliberate joint effort by the elites to stabilize the system."[21] Rejecting the term "grand coalition" to describe the cooperative efforts of all segment representatives on behalf of the system, Lijphart instead invoked a term he attributed to Ralf Dahrendorf—"elite cartel"[22] —which he used in his most succinct definition of consociational democracy: "Consociational democracy means government by elite cartel designed to turn a democracy with a fragmented culture into a stable democracy."[23]

Lijphart used the terms "elite cartel" or "cartel of elites" five times in his 1969 article.[24] The elite cartel had to meet four "requirements" in

[19] Ibid., 211.

[20] Ibid., 212, emphasis in original.

[21] Ibid., 213.

[22] Ibid. Lijphart accurately cites Dahrendorf's book *Society and Democracy in Germany* but fails to mention that Dahrendorf's use of the term "cartel of elites" is part of a characterization of Holland, not as a "democracy," but as "a condition of political stagnation . . . an unintentional authoritarianism." See Ralf Dahrendorf, *Society and Democracy in Germany* (Garden City, N.Y.: Doubleday, 1967), 276–77. As we shall see, this appeal to the authority of others for terms and definitions that are then used much differently than intended by their originators has been a regular feature of Lijphart's work.

[23] Lijphart (fn. 5), 216.

[24] Indeed, the introduction of this term in the 1969 article was, without doubt, the most important difference between this version of consociationalism and that presented in the 1968 *Comparative Political Studies* article (fn. 14), in which the term does not appear.

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order to fulfill the all-important role of a cooperative, purposive, cross-segmental leadership group that could bring stability to culturally fragmented democracies. These requirements, "logically implied by the concept of consociational democracy as defined in this paper," are as follows:

> 1. (the) ability to accommodate the divergent interests and demands of the subcultures . . .
> 2. (the) ability to transcend cleavages and to join in a common effort with the elites of rival subcultures . . .
> 3. (a) commitment to the maintenance of the system and to the improvement of its cohesion and stability . . .
> 4. (an) understand(ing) [of] the perils of political fragmentation[25]

Lijphart then surveyed "successful consociational democracies in the Low Countries, Switzerland, Austria, and Lebanon" in order to infer more general conditions "favorable to the establishment and persistence of this type of democracy."[26] He identified eight such conditions, providing illustrations for each from successful consociational democracies or from the work of notable political scientists (for example, Robert Dahl, Quincy Wright, David Easton, Sidney Verba, and Karl Deutsch) whose studies contain observations consistent with the eight hypotheses. According to Lijphart, the "*length of time a consociational democracy has been in operation*" helps produce "habitual" interelite cooperation. "*External threats*" and a "*multiple balance of power among the subcultures*" are both said to be likely to convince elites of the need for unity and cooperation. Meanwhile the task of the elite cartel is made easier if there is a "*relatively low total load on the decision-making apparatus*" and if the different fragments are separated from one another by "*distinct lines of cleavage*" and thus have fewer opportunities for conflict with one another. This last condition has two other favorable consequences for consociationalism: strict compartmentalization fosters the "*internal political cohesion of the subcultures*" and it increases the likelihood that active parties and interest groups will in fact be those organized along subcultural lines, thereby ensuring that the cartel can provide "*adequate articulation of the interests of the subcultures.*" These last three factors make it easier for elites to deliver the acquiescence of their respective group members to the policies of the cartel. Lijphart comments that "(a) final factor favoring consociational democracy is *widespread approval of the principle of government by elite cartel.*"[27]

[25] Lijphart (fn. 5), 216.
[26] Ibid.
[27] Ibid., 217–19, 221–22; all emphases in this paragraph are in the original.

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Lijphart concluded his 1969 article with a reconstituted typology of democracies. He labeled homogeneous and stable democracies, not as "Anglo-American," but as "centripetal." Fragmented and unstable democracies were termed "centrifugal." Stable and fragmented democracies were assigned the name "consociational." Categorization within these three types was not permanent. After the demise of Weimar's centrifugal system, totalitarianism and defeat in war helped create conditions for a centripetal democracy in Germany ("the Bonn Republic"). Although the French Third and Fourth Republics were centrifugal democracies, Lijphart proposed that France could be transformed into a consociational democracy. What would be required, consistent with that which Lijphart emphasized throughout, were French elites willing to substitute "true statesmanship" for the "well-developed capacity for avoiding their responsibility," a characterization he attributes to Nathan Leites.[28]

Consociationalism Hailed

In the late 1960s other scholars also investigated the idea that elites and/or appropriately structured institutions could preserve stability in deeply divided democracies. Their interest was prompted by failed modernization in the Third World seen as resulting from an upsurge of "primordialist" sentiments,[29] by signs that such parochial loyalties were also liable to disrupt putatively modern, industrialized democracies,[30] and by a turn from socioeconomic reductionism to theories focusing on the decisiveness of political leadership and political institutions.[31] Tracing the genealogy of the consociational idea, McRae pointed to Gerhard Lehmbruch's 1967 and 1968 articles about "concordant democracy," W. T. Bluhm's 1968 article on "contractarian democracy" in Austria, Val Lorwin's 1962 article on "vertical pluralism," and his later work, based

[28] Ibid., 223.

[29] The locus classicus for this argument is Clifford Geertz, "The Integrative Revolution: Primordial Sentiments and Civil Politics in the New States," in Geertz, ed., *Old Societies and New States* (New York: Free Press of Glencoe, 1963). In 1970 Donald Rothchild authored a review essay in *World Politics* on pluralism in the Third World. See Rothchild, "Ethnicity and Conflict Resolution," *World Politics* 22 (July 1970). Rothchild approached the problem in terms of "conflict regulation" and "hegemonial exchange" and made only passing reference to Lijphart's 1969 article; but he did cite the term "consociation" as it was used in an essay by M. G. Smith. See Smith, "Pluralism in Precolonial African Societies," in Leo Kuper and M. G. Smith, eds., *Pluralism in Africa* (Berkeley: University of California Press, 1971).

[30] In his *World Politics* article Lijphart cited (p. 220) the influential essay by Walker Connor, "Self-Determination: The New Phase," *World Politics* 20 (October 1967).

[31] On this point, see Samuel Huntington in his foreword to Eric A. Nordlinger, *Conflict Regulation in Divided Societies*, Occasional Papers in International Affairs, no. 29 (Cambridge: Harvard Center for International Affairs, 1972).

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on a 1969 paper, on "segmented pluralism" in Holland, Belgium, Luxembourg, and Switzerland.[32] In a 1969 paper Ronald Rogowski and Lois Wasserspring drew on this body of work to develop a deductive theory of "corporate consensualism."[33]

Lijphart's consociational formula displaced all of these concepts and theories, however. Indeed, in a nod to Lijphart's rhetorical and analytic success, McRae titled his book *Consociational Democracy*, even while including articles by several theorists who had advanced different formulations.[34] In 1974 Hans Daalder published a review essay in *World Politics* entitled "The Consociational Democracy Theme." The essay evaluated six books in three languages published between 1967 and 1974 dealing with the prospects for democratic stability in culturally fragmented societies.[35] Daalder hailed the "typological coining of the model of consociational democracy . . . [as] a major contribution to the literature," a conceptual advance that had acquired what he called the "*imprimatur* of the editors of the Little, Brown Series—Gabriel A. Almond, James S. Coleman, and Lucian W. Pye." This imprimatur was displayed by an advertisement for a 1972 book in that series by Kurt Steiner, *Politics in Austria*: it described the book as "showing 'the Austrian republic's transformation from the "centrifugal democracy" of the interwar period, to the "consociational democracy" of the Great Coalition after World War II, to the current "depoliticized democracy."'" Daalder referred to the various consociationalist and related authors as having formed, via their comments on one another's work, an "incipient school." Their success, he commented, demonstrated "that use of the small exotic case has theoretical significance."[36]

Daalder was the author of several articles on politics in Switzerland and in his own country, Holland. Indeed a substantial element in the warm reception accorded Lijphart for both his 1968 book and 1969 article was the excitement of scholars knowledgeable about one or more

[32] For complete references, see Kenneth D. McRae, "Introduction," in McRae (fn. 13), 2–8.

[33] Ronald Rogowski and Lois Wasserspring, *Does Political Development Exist? Corporatism in Old and New Societies*, Sage Professonal Paper, Comparative Politics Series, no. 01–024, vol. 2 (Beverly Hills, Calif.: Sage, 1971). The authors report that this monograph was first presented as a paper at the 1969 annual conference of the American Political Science Association. See also Ronald Rogowski, *Rational Legitimiacy: A Theory of Political Support* (Princeton: Princeton University Press, 1974).

[34] McRae (fn. 32), 3ff. In his 1977 book Lijphart would use much the same argument as presented by McRae for preferring "consociationalism" to "segmented pluralism," "proportional democracy," and so forth. See Lijphart, *Democracy in Plural Societies: A Comparative Exploration* (New Haven: Yale University Press, 1977), 3–4.

[35] Daalder, "The Consociational Democracy Theme," *World Politics* 26 (July 1974).

[36] Ibid., 606, 609, 605.

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of the small European democracies that provided his empirical base. Daalder described five of the six authors of the books he was reviewing as follows: Lehmbruch, "a German with close knowledge of Austria and Switzerland"; Lijphart, "a Dutchman who received his doctorate from Yale and studied and taught in the United States for twelve years before he returned to the Netherlands to take up a chair at Leiden University"; Steiner, "a Swiss who received a doctorate in Mannheim and is now Professor of Political Science at the University of North Carolina"; Powell, "an American . . . whose present book is based on original fieldwork in Austria"; and Lucien Huyse, "a Belgian who worked in Oxford for some time."[37] Via consociationalism these political scientists were able to capitalize on their specialized knowledge and enhance the prestige of these (often neglected) countries by theorizing the experience of small European states in a way that could expand prospects for democratic stability in the Third World and elsewhere.

Applying consociational theory to these countries, Lijphart and other early contributors to the research program were explicit about the questions they should try to answer. In early-Lakatosian terms, an expanding list of such questions, generated by work done under the rubric of consociationalism or implied by its logic, capable of attracting the attention of skilled social scientists, soluble through systematic empirical research and inferential elaboration of propositions, and constrained by the program's negative heuristic, is called a "progressive problem shift"—the sign of a successful scientific research program. Daalder provides the following list of research tasks:[38]

1. investigating the differential effects of political culture on prospects for consociational regimes
2. exploring boundaries between democratic and nondemocratic consociationalism
3. explaining the decline of consociationalist structures in Belgium, Holland, and Switzerland
4. analyzing the impact on consociationalist systems of cultural and ideological trends in the international arena
5. evaluating the capacity of consociational elites to absorb greater demands for participation from their constituencies

One of the six books included in Daalder's review essay was Eric A. Nordlinger's 1972 monograph *Conflict Regulation in Divided Societies*. Although he praised the book as "a courageous attempt to grapple with

[37] Ibid., 609.
[38] Ibid., 616–20.

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theoretical issues on a comparative basis," he also criticized it as being afflicted with "a somewhat arid quality," the result of the author's lack of "detailed first-hand knowledge of the countries on which he bases his theoretical reasoning."[39] These comments, while understandable as a reflection of Daalder's personal scholarly tastes, are also a harbinger of the degeneration of the consociational research program.

Nordlinger, for his part, took seriously the kind of questions Daalder identified as necessary for the development of consociational theory. Nordlinger tried to define terms (for example, "divided" versus "deeply divided") and specify propositions of consociationalist theory precisely enough to stipulate the empirical claims that were made and then distinguish those supported by available evidence from those that would have to be rejected or left for further study. His approach called for rigorous methods: eliminating explicitly normative elements; searching the histories of the countries usually used to exemplify consociationalism for limits to their corroborativeness; specifying conditions that would lead to consociational failure as well as success; and facing, at least partially, the tension between "democracy" and "elite accommodation" contained within the very idea of "consociational democracy." Nordlinger's effort produced a tightly woven but restrictive theory of the conditions under which "conflict regulation" could be achieved within "divided" but still "open" regimes. This complex set of testable propositions was founded on the parsimonious assumption that both elites and masses act to promote self-interests as they perceive those interests, rather than on the hope or expectation that elites would sacrifice their own interests for the greater good. In essence, Nordlinger theorized consociationalism as a Nash equilibrium that could arise when segmental elites found it preferable to institutionalize their intrasegmental predominance by restraining themselves in competition with one another.[40]

Nordlinger in fact anticipated most of the withering criticisms of Lijphart's work that would come in the mid-1970s and early 1980s—critiques of the imprecision of his terms, the awkwardness of his typol-

[39] Ibid., 620–21.

[40] Among those who have tried to follow Nordlinger's approach, adhering in spirit if not in exact terminology to Lijphart's early work, are Brendan O'Leary, "The Limits to Coercive Consociationalism in Northern Ireland," *Political Studies* 37, no. 4 (1992); John McGarry and Brendan O'Leary, "The Macro-Political Regulation of Ethnic Conflict," in John McGarry and Brendan O'Leary, eds., *The Politics of Ethnic Conflict Regulation: Case Studies of Protracted Ethnic Conflict* (London: Routledge, 1993), 35–37; Donald Rothchild, "Hegemonial Exchange: An Alternative Model for Managing Conflict in Middle Africa," in Dennis L. Thompson and Dov Ronen, eds., *Ethnicity, Politics, and Development* (Boulder, Colo.: Lynne Rienner, 1986); and Ian Lustick, "Stability in Deeply Divided Societies: Consociationalism versus Control," *World Politics* 31 (April 1979).

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ogy, and his mischaracterization of key cases. In turn, Lijphart and other defenders of the consociational research program either ignored or implicitly rejected Nordlinger's approach—an approach based on trading the wide range of consociationalism's applicability (associated with its vagueness) and the high hopes that could be attached to its proliferation, for a tighter, bleaker, more limited, and more strictly positivist set of claims.

Consociationalism Challenged

The two most systematic critiques of the consociational democracy model were those written by Brian Barry in 1975 and by M.P.C.M. van Schendelen, in 1984.[41] Barry's review of the consociational democracy literature concentrated on Lijphart's work, evaluating empirical support for the proposition that politics, in this case "the practices of elite accommodation constituting 'consociational democracy,' was decisive in explaining "stable and effective democracy" in the presence of "isolated and dissensual blocs."[42] Specifically, Barry looked at the argument in Lijphart's 1968 book, that adoption of policies of "accommodation" by segment elites explained stability in Holland, and found it to be tautological. Lijphart's definition of "accommodation," as Barry shows, *includes* the elaborately specified claim that issues dividing polarized blocs are settled by leaders convinced of the need for settlement.[43] In other words, accommodation is simultaneously the successful settling of disputes between cultural fragments and the explanation for stable relations among cultural fragments. Barry demonstrates that whether or not Lijphart's account of Holland is an accurate description, it cannot be an explanation of Dutch political stability.

Barry identifies a similar problem with the term "consociational democracy" in Lijphart's 1969 *World Politics* article. On the one hand, Lijphart presented "government by elite cartel" (that is, consociational democracy) as the explanation for stability, rather than immobilisme, widespread violence, or other forms of instability, in societies with "fragmented political culture(s)." On the other hand, because there are so many separate but content-filled claims built into the "four requirements" Lijphart said were "logically implied" by the concept, classify-

[41] Barry, "Review Article: Political Accommodation and Consociational Democracy," *British Journal of Political Science* 5 (October 1975); M.P.C.M. van Schendelen, "The Views of Arend Lijphart and Collected Criticisms," *Acta Politica* 19 (January 1984).

[42] Barry (fn. 41), 488–89.

[43] Ibid., 479.

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ing a case as "consociational democracy" is tantamount to classifying it as a case of stability and democracy.[44] Thus Lijphart's definition of consociational democracy is too overloaded to do any substantial explanatory work.

Taking consociational democracy as a descriptive category rather than as an independent variable shifts attention to the typology Lijphart substituted for Almond's typology. Lijphart advances his list of possible combinations of stability and segmentation in democracies as exhaustive; by implication, then, *only* consociationalism can account for stable democracy in culturally fragmented societies. Once again Barry points to the tautological basis for the "truth" of Lijphart's claim that consociational democracies are *both* "'fragmented but stable democracies' and countries with 'government by elite cartel.'" So just as Almond's typology ruled out "*any* 'fragmented but stable' democracy," so Lijphart's typology ruled out any "fragmented but stable" society where "government by elite cartel" was not present.[45]

Barry then considered the accuracy of the case descriptions offered by Lijphart and other consociational theorists, basing his account of the cases on works authored by scholars participating in, or most in sympathy with, the consociationalist model. Citing the mildness of ethnic conflict in Switzerland, the use of majoritarian techniques such as binding referenda, and successful challenges to elite decisions, Barry observes that Switzerland is neither deeply divided, nor conflictual, nor consociational in design, nor stable because of undesigned consociational practices. "Switzerland," he concludes, "fails to fit [the model of consociational democracy] at every point."[46] And although Barry agrees with Lijphart and others that the Austrian Second Republic (1945–66) did conform closely to the elite cartel model, he challenges their proposition that consociationalism was a necessary condition for Austrian political stability in the Second Republic. According to Barry, the Austrian Catholic and socialist political parties were really not all that divided and conflict prone. Rather, the level of hostility between competing parties (Lijphart calls them "*laagers*") was low, and the restraining influence of the masses on their leaders was substantial—the opposite of what is entailed by the consociational model. Barry also points to confounding factors—economic prosperity and a European environment much more conducive to parliamentary democracy after

[44] Ibid., 480.

[45] Ibid., 481, emphasis in original.

[46] Ibid., 486. Barry's discussion of Switzerland is based on work by Jürg Steiner and William R. Keech.

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World War II than before it—that preclude using the Austrian case to confirm robust claims about the efficacy of elite choices and of political institutions for explaining increased democratic stability in that country after the war.[47]

Holland and Belgium remained, according to Barry, the two "classic cases" of consociational democracy. Accepting that consociationalism in those two countries "has contributed to the stability and general acceptability of the system in a way that a more majoritarian pattern of conflict-management would not," Barry notes how similar these two cases are—contiguous countries with divisions rooted in "the church-state question and the challenge posed by the rise of the working-class movement."[48] But, Barry argues, these divisions cannot be used to establish the applicability of the consociational model for societies divided into ethnic fragments. Thus, in Belgium the parties involved in consociational arrangements do *not* represent Flemish and Walloon sentiments; indeed, they actively oppose efforts to mobilize these linguistic/ethnic/regional identities. "Except where it is the prelude to peaceful fission of the state," Barry warns against encouraging the formation of monolithic, politically antagonistic communities as a means of establishing consociationalism in ethnically divided societies. Leaders of such ethnically defined groups, he cautions, are likely to be caught in outbidding struggles with rivals within their segments, creating conditions for "potential civil war or of civil war averted by effective oppression by one group of the other."[49]

In perhaps the most comprehensive critique of Lijphart's theory ever published, the Dutch political scientist M.P.C.M. van Schendelen raised a question only alluded to by Barry: is the Netherlands itself really a valid illustration of consociational democracy?[50] The analysis focused on three of Lijphart's works: the 1968 book, the 1969 *World Politics* article, and the 1977 book, *Democracy in Plural Societies*, the last

[47] Ibid., 490–500. Barry's analysis of Austria relies on work by P. G. J. Pulzer, William T. Bluhm, and G. Bingham Powell.

[48] Ibid., 501.

[49] Ibid., 504–5. Lorwin, Nordlinger, and Horowitz made similar assessments of the inappropriateness of consociationalism for societies that are "deeply divided" as opposed to divided in the manner of the small European democracies. Each therefore doubted that the model could usefully be applied to most Third World cases or to countries where class divisions coincided with cultural fragmentation. Val R. Lorwin, "Segmented Pluralism: Ideological Cleavages and Political Cohesion in the Smaller European Democracies," in McRae (fn. 13), 35; Nordlinger (fn. 31), 92; and Donald L. Horowitz, *Ethnic Groups in Conflict* (Berkeley: University of California Press, 1985), 571–72.

[50] See van Schendelen (fn. 41); and Barry (fn. 41), 501. For other effective critiques, see Adriano Pappalardo, "The Conditions for Consociational Democracy: A Logical and Empirical Critique," *European Journal of Political Research* 9 (December 1981); and Sue M. Halpern, "The Disorderly Universe of Consociational Democracy," *West European Politics* 9 (April 1986).

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a reworked presentation of the theory and the work thereafter cited by Lijphart as his authoritative statement on the subject. Van Schendelen argued that the Dutch literature on the political history and dynamics of the Netherlands revealed a story very different from that presented by Lijphart to English-speaking scholars.[51] A close reading of this literature, he argues, shows that Lijphart's use of evidence in his depiction of Dutch political life from 1917 to 1967 as a successful, stabilizing consociational democracy was highly selective.[52]

Van Schendelen cites the Dutch sociologists J. P. Kruyt and H. Verwey-Jonker, who argued that the "pillarization" used by Lijphart to establish the segmentation of Dutch society was "seriously weakening" even in the 1950s, that cross-denominational cooperation was increasingly evident, and that coherent political subcultures were dissolving as "increasing numbers of people abstained from convergent memberships and preferred cross-cutting memberships."[53] Work by other scholars, including Lijphart himself, is said to show significant crosscutting cleavages.[54] Van Schendelen thus raises serious questions about Lijphart's characterization of the Netherlands as a culturally fragmented society saved from democratic instability only by consociationalism.[55]

At the heart of consociational theory, and of Lijphart's formulation in particular, is the decisive influence of elites committed to exercising restraint and accommodation in order to achieve system stability. While Hoogerwef, according to van Schendelen, did portray distances between Dutch elites as smaller than at the mass level, Scholten accounts for backroom deals among Dutch political leaders as stemming from a typical and nearly universal inclination of elites to use secrecy, depoliticization, and summit diplomacy to advance their own self-interest. Rather than imagine, à la Lijphart, a kind of altruistic, "system level" motive in the cooperative behavior one observes in the Dutch case, van Schendelen, citing Daudt, points to the "structure of the electoral system" (extreme proportional representation, with 10–14 parties in the Parliament) as creating incentive enough to explain cooperative behavior and the formation of large coalitions.[56]

[51] Van Schendelen (fn. 41), 48.
[52] Ibid., 42.
[53] Ibid., 35.
[54] See Lijphart (fn. 7), 89–92.
[55] Van Schendelen (fn. 41), 36. See also Ronald A. Kieve, "Pillars of Sand: A Marxist Critique of Consociational Democracy in the Netherlands," *Comparative Politics* 13 (April 1981); and Ilja Scholten, "Does Consociationalism Exist? A Critique of the Dutch Experience," in Richard Rose, ed., *Electoral Participation: A Comparative Analysis* (Beverly Hills, Calif.: Sage Publications, 1980).
[56] Van Schendelen (fn. 41), 37–38.

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As for stability, van Schendelen agrees that as measured by protests, riots, and violent turmoil, Holland has been stable, but this was so already in 1917, before the consociational system was said to have gone into effect. Measured in the way that postwar Italy was measured by both Lijphart and Almond, however—by counting instances of government turnover—the Netherlands registers great instability. It had twenty-three different cabinets between 1917 and 1967, with caretaker governments ruling for an average of twenty-seven days per year. The reality, according to Kieve, was "political immobilism and deep inter-elite divisions." "Immobilism," of course, was the very term used by Almond and Lijphart to characterize the outcome of cultural fragmentation combined with democracy in the *absence* of consociationalism.[57]

Van Schendelen's survey of research on the democratic nature of Dutch political life poses further problems for the theory. According to the standard of regular elections, he notes, Holland has been a democracy. But, as Lee Dutter puts the question directly: "Can the Netherlands really be called a democracy?"[58] Specifically, at what point, based on practices of exclusion, control, closed decision making, limits on majority influence, and other devices used by elite cartels, should a consociational system, however stable, be judged to no longer be democratic? Daalder, for his part, describes the "closed elite system" in Holland as an "oligarchy."[59] And van Schendelen cites research depicting the Dutch population as resenting its victimization by parliamentary elites who do not represent its interests.[60] While not going so far as to describe the country as nondemocratic, van Schendelen echoes Barry's criticism, that Lijphart packs too much unacknowledged theory into his typology. Van Schendelen challenges Lijphart's position that Dutch political life should not be measured against an ideal model of democracy. He holds, contra Lijphart, that if consociationalism is less than ideal democracy but still democracy, then it should not be cast as a typological category of democracy but as a variable. This would recognize the possibility that under certain circumstances one could judge a case to be no longer democratic, regardless of how consociational it is.[61]

Van Schendelen seconds Barry's criticism of circularity in the basic presentation of consociationalism and cites work by Boynton and

[57] Ibid., 38–39.
[58] Ibid., 39.
[59] Ibid.
[60] Van Schendelen cites works in Dutch by J. van Putten, Geismann, and H. Daudt; see van Schendelen (fn. 41), 39–40.
[61] Ibid., 40.

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Kwon that identifies the same problem.[62] But van Schendelen's catalog of Lijphart's questionable claims about Dutch political life reflects his interest in a problem that Barry did not stress: the problem of imprecision in the definition of key concepts and the various conditions said to be conducive to consociationalism. It is Lijphart's vagueness in this regard that encourages selective use of evidence in the stylization of case histories, prevents formulation of objective indicators of key variables, and precludes empirical testing.[63]

We have already seen how Lijphart's 1968 and 1969 definition of "accommodation" or "consociationalism" contained the theory he sought to promote. However, when it came to key elements of the theory—"democracy" and "stability" (the dependent variables) and "pluralism" or "cultural fragmentation" or "divided" or "deeply divided" societies (a necessary condition for applying the theory)—Lijphart failed to offer definitions of any kind. Only in his 1977 book did Lijphart respond to this criticism, offering explicit definitions of key terms—explicit, but far from precise.

"Democracy," he wrote, "virtually defies definition. Suffice it to say that it will be used here as a synonym of what Dahl calls 'polyarchy.' It is not a system of government that fully embodies all democratic ideals, but one that approximates them to a reasonable degree."[64] This is a definition that seems impossible to operationalize: what is meant by "reasonable"? what "democratic ideals" does he have in mind? Van Schendelen adds that Dahl's concept makes an oxymoron of consociational democracy: "In a polyarchy competition between the elites is, more than anything else, essential; in a consociation basically the opposite, namely intense collaboration, is crucial."[65]

Lijphart's definition of stability is similarly problematic. He begins by asserting that "[p]olitical *stability* . . . [is] . . . an almost equally difficult and ambiguous term."[66] Citing usages by Harry Eckstein and Leon Hurwitz, he explains that stability "will be used in this study as a multidimensional concept, combining ideas that are frequently encountered in the comparative politics literature: system maintenance, civil order, legitimacy, and effectiveness . . . [j]ointly and interdependently, these

[62] Ibid., 34, 43.

[63] On the general problem of calibrating the stylization of cases to make comparison possible but avoid selection bias, see Ian S. Lustick, "History, Historiography, and Political Science: Multiple Historical Records and the Problem of Selection Bias," *American Political Science Review* 90 (September 1996), 605–18.

[64] Lijphart (fn. 34), 4.

[65] Van Schendelen (fn. 41), 32.

[66] Lijphart (fn. 34), 4, emphasis in original.

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four dimensions characterize democratic stability." Again one encounters imprecision and the associated difficulty of developing coding rules to distinguish "unstable" from "stable" cases. Moreover, van Schendelen notes, Lijphart's invocation of Eckstein and Hurwitz as authorities for his usage is unsupported by any claims made by those authors. Hurwitz, in the work cited by Lijphart, discussed "not one but several conceptualizations of stability" concluding that "the concept of political stability remains as elusive as other abstract concepts," while Eckstein warned that his "four dimensions of stability" could be treated only as "tentative ideas" and as "provisional."[67]

Lijphart also drew on Eckstein to define "plural society." "Segmental cleavages," according to Lijphart, "may be of a religious, ideological, linguistic, regional, cultural, racial, or ethnic nature. A further characteristic . . . is that political parties, interest groups, media of communication, schools and voluntary associations tend to be arranged along the lines of segmental cleavages."[68] But as van Schendelen, citing Jürg Steiner and G. Bingham Powell, points out, with so many different kinds of cleavages, listed as dimensions which "may" determine whether the society is plural, and which could, via so many organizational expressions of the plural nature of society, "tend" to be arranged along segment lines, it becomes impossible to distinguish societies that Lijphart would classify as plural from those he would not classify as such. "On the basis of which criterion," van Schendelen asks, "can one say that some division is *not* a cleavage and that a cleavage is *not* segmental?"[69]

Lijphart's treatment of crosscutting cleavages is a spectacular illustration of the problem created by this vague definition of "fragmented," "segmented," or "plural." As we saw at the outset, the theory of consociationalism was advanced to explain the "puzzle" of democratic stability in the absence of crosscutting cleavages. But as one Dutch reviewer commented in response to Lijphart's first book, "Personally I think that the cleavages in the Netherlands are, generally speaking, more crosscutting than mutually reinforcing."[70] Indeed, Lijphart himself seems to have had difficulty distinguishing between a society marked by crosscutting cleavages and one that he would classify as "plural," "segmented," or "fragmented." In *The Politics of Accommodation* he described "crosscutting links" in Holland: "[R]eligious and class lines in Holland

[67] Hurwitz and Eckstein as cited in van Schendelen (fn. 41), 33.
[68] Lijphart (fn. 34), 3–4, emphasis in original.
[69] Van Schendelen (fn. 41), 31, emphasis in original.
[70] Andries Hoogerwerf, review of *The Politics of Accommodation*, *American Political Science Review* 62 (December 1968), 1350.

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run at right angles to each other . . . [and are] . . . an important factor which . . . at least mitigates the effects of the poor integration of Dutch society." As a result of these crosscutting cleavages, Lijphart concluded, "party leaders are under constant cross pressures from different wings of the party, which predispose them to moderation and compromises in both intraparty and interparty relations."[71]

In his 1969 *World Politics* article Lijphart omitted any discussion of crosscutting cleavages as a factor conducive to consociational success, whereas in 1977 he revived the idea, though his thinking remained obscure. Although "the theory of consociational democracy does not rely on the presence of crosscutting cleavages as a primary explanation of the political stability of plural societies . . . crosscutting divisions of equal or unequal intensities are a factor of subsidiary importance, and they may or may not be favorable to consociationalism." Thus, Lijphart's 1977 multivalent definition of "segmented" does not seem to eliminate confusion, since a factor (crosscutting cleavages) contrary to a necessary condition (segmentalism) is listed as a contributing element in the theory (consociationalism). Confusion continues to mount in the book's conclusion where "pluralism," characterized by crosscutting cleavages in much of the literature cited by Lijphart, is employed as a synonym for the "segmentalism" or "fragmentation" of a "plural" society marked by the *absence* of crosscutting cleavages.[72]

As noted above, Lijphart listed eight (as I count them) conditions as favoring the success of consociationalism. Having dropped five and added three or four (depending on how one counts), Lijphart ends up in 1977 with a list of six or seven conditions conducive to consociational success (including some crosscutting cleavages).[73] Van Schendelen remarks that all of the conditions are "empty," because none of them can be tested in any practical way.[74] This is so because, as Lijphart him-

[71] Lijphart (fn. 7), 88–89, 205.

[72] At the end of the book Lijphart presents a graph treating pluralism as a variable that, as it increases, slightly increases the likely success of consociationalism. As the degree of pluralism increases beyond a certain point, however, the expectations of consociational success decline (though not as rapidly as expectations of successful centripetal democracy) until such a high degree of pluralism is reached that any kind of democracy, including consociationalism, is doomed. Lijphart (fn. 34), 237. See also fn. 77.

[73] In 1985 Lijphart wrote that in his 1977 book, *Democracy in Plural Societies*, he "listed nine conditions conducive" to consociational democracy. See Lijphart, *Power-Sharing in South Africa*, Policy Papers in International Affairs, no. 24 (Berkeley: University of California, Institute of International Studies, 1985), 114. In the pages he cites there are indeed nine subheadings for topics treated as such conditions. But these nine include "crosscutting cleavages" which he describes on the next page of this 1985 book as "*not* a favorable condition," emphasizing that "just because it was a subheading doesn't mean I meant it as a favorable condition." It is thus difficult to tell exactly how many conditions he did list as "conducive" in 1977.

[74] Van Schendelen (fn. 41), 34.

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self emphasizes, neither separately nor jointly do these conditions ensure the presence or success of consociationalism.

Van Schendelen concludes that Lijphart probably cared little about the empirical validity of his theory or about evaluating its robustness systematically. Citing the request from the Legislative Assembly of KwaZulu for Lijphart's help in designing a consociational solution for South Africa, van Schendelen repeated Barry's warning against overzealous application of the model as a normative guide for the construction and reconstruction of states. Lijphart, he opines, "seems to attach more value to the theory's potential for engineering societies than to any other criterion of science." Thus does van Schendelen explain Lijphart's movement away from his early notion of consociationalism as a specifically Dutch phenomenon, or as an unusual constellation of practices and circumstances found in several small European democracies, and toward a much more expansive view of consociationalism as a formula to be applied in almost any country facing political instability stemming from intergroup conflict.[75]

Lijphart Responds: The Shift to Late Lakatos

Lijphart and his followers made some effort to respond to criticisms regarding the analytic rigor of his concepts and models, errors of inference, and interpretations of evidence.[76] But Lijphart's primary reaction was based on normative appeals accompanied by an epistemological shift and an evangelical prescription of consociationalism for most if not all politically troubled societies: "Consociational theory asserts that the probability of successful majoritarianism is *always* much smaller than that of successful consociationalism. Hence it *never* makes sense to recommend majority rule instead of consociational democracy for a plural society."[77] In the terms this essay has used, Lijphart abandoned early-Lakatosian criteria for good social science (progressive iterations of testable and increasingly robust explanatory claims within a stable

[75] Ibid., 44, 26.

[76] See, for example, Conway W. Henderson, "Comment: Consociational Democracy and the Case of Switzerland," *Journal of Politics* 43 (November 1981), 1232–35; and Pierre du Toit, "Consociational Democracy and Bargaining Power," *Comparative Politics* 19 (April 1987).

[77] Lijphart (fn. 73), 101, emphasis added. The extremely broad referent for this statement derives in part from the expansive and ambiguous definition of "plural," a term, he observes, that "is just about as difficult to define precisely as the concept of democracy." Explaining that societies vary from 0 percent to 100 percent plural, Lijphart warns that "the degrees of pluralism are still not exactly measurable, and a judgment of the extent to which a given society satisfies each of the criteria is necessarily 'impressionistic' but unfortunately no better method is available in the current stage of development of the social sciences"; see Lijphart (fn. 73), 87.

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framework of presuppositions and definitions) for late-Lakatosian criteria (effectiveness in meeting the goals, whether personal or political, of the leaders of the research program).

Lijphart's most systematic and explicit response to his critics came in his 1985 book *Power-Sharing in South Africa*. However, most of the points made by Lijphart pertain to side comments made by critics or to interpretations that defang their main contentions. David D. Laitin characterized Lijphart's technique in this book as resembling that of "Cyrano de Bergerac, sword in hand, killing off all comers. This battle is truly valiant even though the reader hardly hears the full voices of the critics; they appear to be advancing on Lijphart without weapons."[78]

For example, defending consociational democracy theory against criticism that it should not automatically be considered "democratic" because of control over and exclusion of masses from meaningful political participation, Lijphart ignores his own definitional use of the concept of "elite cartel." He focuses instead on the element of secrecy in consociational politics, averring that secrecy figures in the political activity of elites in all societies. He thereby shifts attention to a minor, but related point and away from the more fundamental problem. The effect is to preserve consociationalism's attractive democratic reputation without revisiting the problem of defining it. Lijphart is content to conclude summarily that consociational democracy "has been amply defined and . . . nothing needs to be added to it." He thereby avoids the need to square his declaration that "[t]here is nothing in consociationalism that true democrats have to be ashamed of . . . [i]t is fully democratic . . . just as democratic as majoritarianism"[79] with his earlier description of "governance by elite cartel" as requiring "elite dominance," "firm control," and "oligarchical control" within segments comprising a consociational polity.[80]

With prescription and engineering rather than explanation as the main objectives, and with widespread implementation as the mark of success, what seems to have come to matter most to Lijphart was the

[78] David D. Laitin, "South Africa: Violence, Myths, and Democratic Reform," *World Politics* 39 (January 1987), 263.

[79] Lijphart (fn. 73), 110–11, 84n, 109.

[80] Lijphart (fn. 7), 140. In 1977 Lijphart had already indicated his discomfort with this logically important but normatively unattractive aspect of consociationalism by complaining that Brian Barry's arguments were "concentrat(ing) too much on the problem of elite control of the segments"; see Lijphart (fn. 34), 234. It should be noted that in his 1985 book Lijphart repeatedly blames the concerns of critics on exaggerations and errors found in his early work on consociationalism, but he does not explain what he means, thus enabling him to claim that the hard core of his research program remains intact while making it impossible to separate out for any given time the claims, definitions, and hypotheses he still stands by from those that he has discarded. See Lijphart (fn. 73), 83, 115n.

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normative and political value of arguments and evidence likely to rally support for or reduce opposition to "consociationalism" as a political formula. Hence, while admitting its "imperfectly democratic nature," Lijphart declared that he "still fully support[ed] the proposition that plural societies do not have two alternative democratic options and that consociationalism is their only democratic option." Instructively, his evaluation of what he described as a nondemocratic, "quasi-consociational" constitutional reform proposal developed by the South African government in 1983 turned on assessing its inclusion of some "consociational elements" as a "favorable development" against the "effect of giving consociationalism a bad name."[81]

Lijphart works hard in his 1985 book to adapt consociational theory to a new academic environment. Thus, in the late 1960s and early 1970s he offered consociationalism as a meliorative response to a world of states riven by obvious and essentially noncontingent cultural divisions,[82] citing work by Walker Connor and others. He directed the positive heuristic of the research program toward the problem of variation in relations among segments whose identity was obvious and whose existence could be taken for granted. Although the 1985 book retains a reference to "segmental divisions" as "social facts and not a mere fiction serving the political elite," it avoids any commitment to the essentialist idea of "primordial" identities. Instead Lijphart adopts the (now dominant) constructivist view of cultural identities—that group identities are not primordially given and unproblematically evident but are contingent upon and identifiable from political processes. Accordingly, he argues that South Africa is plural not because of a division between a black majority and a white minority (difficult for consociationalism) but because of divisions among many ethnic/tribal groups, both black and white. This attempt to extend the reach of consociationalism is, however, based on propositions that in fact contradict the fundamental assumptions that undergird it. This departure from the research program's negative heuristic, though unacknowledged, produces multiple contradictions. "Pluralism" is thus presented variously as (1) a variable that can range in any society from 0 percent to 100 percent (2) a categorical fact (a society is either plural or not) whose appreciation precedes determination, through elections, of the *particular* segments which endow the society with its "plural character" *and* (3) a "social

[81] Lijphart (fn. 73), 109, 63.
[82] Lijphart (fn. 5), 220; idem (fn. 34), 16.

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fact" whose particular lineaments present themselves clearly because of the obvious and noncontingent character of real segmental divisions.[83]

The 1985 book contains many examples of Lijphart's substantial shift in epistemological commitments and scholarly purposes. But inferring this shift from specific instances of argument, terminology, and evidence use is not necessary. Despite categorical affirmations of the positivist credentials of consociational theory ("The theory of consociational democracy is a basically valid empirical theory"),[84] Lijphart explicitly rejects precision, consistency, and measurement against evidence as tests of its value.

> Researchers who insist on the exact measurement of their variables are likely to get bogged down in measurement problems, and hence they may never engage in the more important scientific task of establishing empirical relationships among the variables. It is much better to use a simple, rough, and indeed "impressionistic" twofold or threefold classification of the variables—for instance, a homogeneous versus a plural society, consociational versus majoritarian democracy, and civil peace versus violence—and to relate these variables to each other than to spend all of one's time in a probably futile effort to find exact measurements. Van Schendelen's unrealistically high standards of empirical research would have made the development of consociational theory as well as the formulation of consociationalism as a normative model impossible. Methodological perfectionism is extremely debilitating for political theory and practice.[85]

This is a standard in opposition to the standards employed by Nordlinger, Barry, and van Schendelen. It even contradicts those Lijphart himself employed, using evidence and careful exegesis of theoretical and inferential logics to launch consociational democracy theory as a solution to logical and empirical problems in the pluralist theories of Bentley, Truman, and Almond. Lijphart's struggle over time thus emerges as mainly political, to affirm consociationalism as valid (almost) regardless of its scientific status, because it serves the ends he values. Accordingly, Lijphart gives full-throated endorsement to consociationalism as a "normative model" for engineering a better state of affairs than that possible according to any other formula, democratic or otherwise.

In their extended reviews of *Power-Sharing in South Africa*, Laitin and John McGarry and S. J. R. Noel make this point, arguing that it follows from Lijphart's previous writings that South Africa would *not* be an encouraging site for consociationalism. Laitin characterizes Lijp-

[83] Lijphart (fn. 73), 68, 87, 108.
[84] Ibid., 83.
[85] Ibid., 87–88.

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hart's effort as an exercise in Sorelian myth making and agrees with van Schendelen's description of Lijphart's propositions as "empty." "If conditions are favorable for consociationalism," Laitin observes, Lijphart's theory leads him to propose a consociational system; if conditions are unfavorable, he proposes the same."[86] Hence, Lijphart fails to consider the threat of black majority violence as a decisive factor in the political equation, an omission Laitin attributes to the real (nonanalytic) purpose of Lijphart's project—to create a politically useful resource, a utopian "consociationalist" myth, for societies (including South Africa) he thinks need to believe in the peaceable attainment of democracy stability.[87] Along these same lines McGarry and Noel interpret Lijphart's insistence on applying consociationalism to South Africa as evidence that

> the original descriptive and explanatory purpose of Lijphart's theory has come to be largely overshadowed by its prescriptive and normative implications . . . that . . . Lijphart himself . . . has become progressively more concerned with specifying the circumstances in which consociational democracy is both possible *and the most appropriate* form of government.[88]

In this late-Lakatosian posture, methodology is a political weapon rather than a tool for improving theory, allowing Lijphart to shift between impressionistic and precise uses of definition and evidence. Employing precise use of evidence and analysis to defeat alternatives to his position, he delegitimizes this technique as a basis for criticizing consociationalism and makes every possible allowance for the "impressionistic" nature of social science where evidence or logic is inconvenient. Ignoring the principle of parsimony, he protects his research program via ad hoc and inconsistent amendments to what had been its "hard core," basing himself on appeals to the authority of scholars who reviewed his work positively, to the sheer number of consociationalist studies of various countries, and to the attention accorded the theory by South African politicians and constitutional designers.[89]

[86] Laitin (fn. 78), 265.

[87] Ibid., 275–77.

[88] McGarry and Noel, "The Prospects for Consociational Democracy in South Africa," *Journal of Commonwealth and Comparative Politics* 27 (March 1989), 4–5, emphasis in original. Lijphart's lack of restraint in his application of consociationalist formulas and his singularly close identification with the term create problems for scholars such as McGarry and his collaborator, Brendan O'Leary, who have sought to use consociationalist propositions rigorously and selectively. See their interchange with one critic who uses Lijphart's shifting meanings to criticize their work. Paul Dixon, "The Politics of Antagonism: Explaining McGarry and O'Leary," *Irish Political Studies* 11 (1996), 130–41, 155–59; McGarry and O'Leary, "Proving Our Points on Northern Ireland (and Giving Reading Lessons to Dr. Dixon)," *Irish Political Studies* 11 (1996), 142–54.

[89] At the beginning of the chapter written as a response to his critics, Lijphart includes footnotes listing 104 separate published studies of various countries "from a consociational perspective." See Lijphart (fn. 73), 84n.

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Conclusion: Politics Over Evidence in Lijphart's India

This review of the consociationalist research program, its trajectory, and the changing methodological rationale of its leading exponent might well have ended in the mid-1980s. There was a general slackening of interest in the idea after that, as increasingly influential constructivist, hegemonic, and rational choice approaches came to challenge various key elements of Lijphart's consociationalism.[90] First, scholars seem increasingly skeptical that cultural group leaders generally prefer self-restraining accommodationism to outbidding aggressiveness. Second, partisans of democracy have been dissatisfied with a notion of democracy requiring low levels of popular participation. And finally, collapse or transformation of previously exemplary consociational countries and confusion over how deeply divided a society must or should be for consociational institutions to work have made it difficult to restore focus to the consociational program. In recognition of these trends, perhaps, and desiring to promote the "essence of consociationalism . . . the search for broadly acceptable compromises," Lijphart himself developed overlapping notions of "power sharing" and "consensus democracy" that could be applied to societies which were not "deeply divided."

Nonetheless in the mid-1990s Lijphart again raised the banner of consociational evangelism. In 1995 he hailed the "thoroughly consociational character" of the Framework for Accountable Government in Northern Ireland proposed by the British government.[91] More spectacularly, in a recent article in the *American Political Science Review*, he acclaimed India as a dramatic confirmation of consociational theory. In fact, the article constitutes an excellent illustration of how Lijphart's move toward an "impressionistic" social science has allowed him to use the language of scholarly rigor, absent its substance, to advance his political objectives in late-Lakatosian style.

A classic element in Lakatos's theory was a reconsideration of the role of "crucial experiments"—observations interpreted as dramatic victories and/or decisive defeats for scientific theories or families of theo-

[90] According to the University of Michigan's index of dissertation abstracts, consociationalism was the focus of twenty-seven Ph.D. dissertations between 1980 and 1989 (more than half of these appeared in the period 1980–82.), but only of eight from 1990 to 1995. No dissertations on consociationalism were listed for 1994, 1995, or the first quarter of 1996.

[91] Arend Lijphart, "Consociational Theory and the Case of Northern Ireland" (Paper presented at the annual convention of the American Political Science Association, Chicago, August 31–September 3, 1995), 5. Two decades earlier Lijphart judged Northern Ireland as unfit for consociational treatment, recommending partition instead. See Lijphart "Review Article: The Northern Ireland Problem—Cases, Theories, and Solutions," *British Journal of Political Science* 5 (January 1975).

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ries. Although he doubted that these events were ever quite what they seemed or were made to seem, (early) Lakatos did ascribe real importance to the ability of a research program to transform an anomaly into a profoundly confirming case of its hard-core theoretical propositions.[92] Despite what I have shown as Lijphart's abandonment of the epistemological premises of this approach to social science, this is precisely what he sought to do in his recent treatment of India.

Lijphart sets the stage by characterizing "consociational theory" as having had "a strong influence on comparative politics," of having "spawned a vast literature," and as having "become a widely accepted conceptual framework for the analysis of democracies that can be regarded as the prototypes of power-sharing." Its "otherwise unblemished empirical validity," however, has "remained vulnerable on one major count: the glaring exception of India."[93] Lijphart's purpose here is to eliminate that one "exception" to the theory and turn it instead into spectacular corroboration. For this, he needed two basic elements. First, having previously described India as neither "consociational" nor a "long-term democracy,"[94] he needed to recode it as both. Second, since India (according to Lijphart) is now increasingly unstable, the period of Indian consociationalism must be seen as having occurred prior to the onset of this instability. With these two elements in place, the explanatory power of consociational theory can be revealed by the onset of instability in Indian democracy *after* the abandonment or degradation of consociational practices.

Despite Lijphart's assertions to this effect, however, what the exercise actually shows is how an impressionistic conception of social science (such as Lijphart articulated in 1985) can afford expansive rhetorical opportunities to a research program with aspirations for near universal application. Also associated with this impressionistic approach are vague and elastic coding rules that permit extreme selectivity in the use of evidence. His argument regarding India is summed up as follows:

> The evidence clearly shows India has always had a power-sharing system of democracy. . . . As Indian democracy has become less firmly consociational, intergroup tensions and violence have increased. If this reinterpretation is correct—as I shall try to demonstrate in this article—India is no longer a deviant case for consociational theory, and, in fact, becomes an impressive confirming case.[95]

[92] Lakatos (fn. 1), 154–74.
[93] Lijphart (fn. 6), 258, 259.
[94] Lijphart (fn. 34), 225; and idem (fn. 73), 39.
[95] Lijphart (fn. 6), 259.

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Lijphart classifies India, operating under the "Congress Party system," as "fully" consociational in its first two decades and "basically" so "in more recent decades."[96] This classification emphasizes as decisive the consensual and broadly aggregative, representative character of the Congress Party.[97] This change in coding rules was essential because every defining feature of consociationalism listed in the 1969 article was absent in India. Thus, Indian politics featured majoritarian and other distinctly nonconsociational institutions—centralized federalism, one-party majority cabinets, a "highly disproportional electoral system," political parties unrepresentative of the groups whose conflicts threaten civil peace (Muslims versus Hindus; lower- versus upper-caste Hindus), and rule by powerful prime ministers (Nehru in particular), rather than by groups of accommodating elites.

To classify the Indian case as consociational, Lijphart had to ignore or minimize these factors and instead look to practices that could be seen as consistent with some element of consociationalism or at least as "conducive" to consociationalism in any one of Lijphart's various formulations. For example, he interpreted the Congress Party's inclusiveness and "political dominance" and the heterogeneous cultural complexion of many cabinets as indicating "a consociational grand coalition,[98] even though the party never won a majority of the Indian vote and even though cabinets had token, not representative, membership from different cultural groups. Although in the South African case Lijphart explains that the parties produced by elections would ipso facto indicate which homogeneities and cleavages made the society "plural," in the Indian case he ignores the absence of parties representing these groups. Instead he takes the state's acceptance of religious differences in the domain of personal law, government financing of religiously based school systems, and conformance of some Indian state boundaries to linguistic patterns to meet the consociational standard for segmental autonomy. India's plurality-based, majoritarian electoral system can be made "consociational" by attributing primary importance, not to religion or caste, but to the linguistic factor, according to which many (linguistic) minorities have their own states and predominate in them by virtue of nonproportional elections. Likewise, the prin-

[96] Lijphart (fn. 6), 262.

[97] It should be noted that in his *World Politics* article Lijphart had criticized Almond for using the presence of "aggregative" political parties to classify democracies. To make his point, he identified such parties as operating within one segment of a consociational polity (the Belgian Catholic party), as well as in a nonconsociational "centrifugal" democracy (Italian Christian Democrats). Lijphart (fn. 5), 210.

[98] Ibid., 260.

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ciple of majority veto is said to have resulted in a language policy that did not establish Hindi as the exclusive official language. No constitutional establishment of minority veto is said to be necessary because of crosscutting cleavages (here appearing in a crucial supporting role for consociationalism) that divide the Hindu majority by language, caste, and sect.[99]

Thus, in 1996 Lijphart found a way to place India in the consociational camp. In 1969, he described "the essential characteristic of consociational democracy" as "not so much any particular institutional arrangement as the *deliberate joint* effort by the *elites* to stabilize the system.[100] Lijphart's 1996 description of India shows how dramatically the meaning of consociationalism had to be changed. In this version India is consociational even though Lijphart could find no deliberate or joint stabilizing effort by elites and even though he describes the mechanism that produces stabilizing practices as "incremental and sometimes haphazard" and as implemented, not by a cartel of elites, but by one great man—Jawaharal Nehru. Despite Nehru's failure to grasp the (putatively) consociational logic of the system, it is his leadership, Lijphart declares in 1996, that "from the perspective of consociational theory," makes "the adoption of power sharing by India and its maintenance for nearly half a century . . . not at all unexpected or surprising."[101]

Lijphart then lists nine conditions said to favor the development and success of consociationalism. As with South Africa in 1985 and Northern Ireland in 1995, he rates India favorably—meeting seven or eight of the nine criteria. And here again loose coding rules lead to systematic selection bias. For example, Indian groups are said to be "roughly the same size" (good for consociationalism) by ignoring the Hindu-Muslim split and characterizing India as divided into "very many minorities" with no predominant groups. An "external threat" to post-independence India (favorable for consociationalism) is also identified as the brief and politically unimportant 1962 "war" with China—not the salient and chronic threat from Pakistan (about which Lijphart says only that wars with that country did not "inflame" Muslim-Hindu relations).[102]

The last step in the argument is to explain instability arising from Muslim-Hindu clashes as a result of a "weakening of power-sharing in

[99] Ibid., 261. With the "numerical majority" of Hindus thus divided, Lijphart declares that there really is no "political majority in India."

[100] Lijphart (fn. 5), 213, emphasis added.

[101] Lijphart (fn. 6), 262.

[102] Ibid., 263.

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India after the late 1960s." The problem here is twofold. The main evidence for a weakening of power-sharing practices is an increase in levels of mass political participation. To the extent that "mounting democratic activism"[103] has made India less consociational, the reader is less apt to accept the original thesis, repeated in 1985, that consociationalism is "fully democratic," since increasing democracy would appear to mean decreasing consociationalism. More importantly, while the traces of "power sharing" or "consociationalism" that Lijphart has gleaned from the whole panoply of Indian political practices mostly pertained to relations among linguistic and regional groups, the instability Lijphart now observes in India is traced almost entirely to surging demands by lower-caste groups and by the powerful mobilization of Hindutva and Muslim sentiments. Since with few exceptions the consociationalist-type practices Lijphart found in India were not directed to the amelioration of these cleavages, the weakening of Indian consociationalism cannot be used to explain the turmoil said to surround them in recent years.

Lijphart's conclusion is that his research on India "strengthens consociational theory by removing the one allegedly deviant case and by showing instead that the crucial case of India is unmistakably a confirming case." With an impressionistic methodological posture, flexible rules for coding data, and an indefatigable, rhetorically seductive commitment to promoting consociationalism as a widely applicable principle of political engineering, it is hardly surprising that Lijphart would assert the "close fit between the Indian case and consociational theory"[104] or that he would suggest that further variants of consociationalism are to be found in Mexico and Japan. The thrust of my argument, however, is that to the extent his triumphal claims about the Indian case are accepted and his suggestions for further research adhered to, what will have been corroborated is not the empirical validity of consociational theory but the late-Lakatosian theory of what can drive a "successful" research program.

[103] Ibid., 263, 264.
[104] Ibid., 266, 265.

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PARADIGMS REVISITED
Productionism, Globality, and Postmodernity in Comparative Politics

By ANDREW C. JANOS*

BY design of the editors of *World Politics* the point of departure for this essay is an article by Albert O. Hirschman published in this journal more than a quarter of a century ago.[1] In it, the distinguished author ranges far and wide, touching on such diverse issues as scholarly style, professional ethics, and theories of knowledge as they apply to social science. As for style, Hirschman favors a "Flaubertian absence" (p. 331) of authors from the pages of their work, letting the narrative speak for itself. As for ethics, he makes no secret of his sympathy for those at the bottom of the global heap of wealth and privilege and of his belief that the social scientist has an obligation to work toward the betterment of their lot.

The centerpieces of the article, however, are two issues in the domain of epistemology. The first of these concerns the utility of paradigms designed to organize scientific research. Although Hirschman himself does not pretend to have a "central epistemological theorem" that would permit us to "differentiate between good and bad theory" (pp. 329–30), his article sounds a warning about "paradigms laying down excessive constraints" (p. 336) and thereby becoming, as the title suggests, a "hindrance to understanding." Although the point is not explicitly made, the context of the article leaves little doubt that he has in mind both the cultural and the dependency theories of political economy, neither of which leaves much room for the material progress of non-Western societies.

The second argument is hardly less important. It concerns the uses of historical analogy for purposes of formulating probabilistic theories.

* The author is indebted to Tadashi Anno, Steven Fish, and Jack Citrin for critical comments on earlier drafts of this article, and to Jeffrey Sluyter-Beltrao for both substantive comment and valuable research assistance.

[1] Hirschman, "The Search for Paradigms as a Hindrance to Understanding," *World Politics* 22 (April 1970).

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Instead of encouraging the practice, so familiar to social science, Hirschman calls upon students of political economy and sociology to "recognize the openendedness of history" (p. 340), to show a "Kierkegaardian passion" for human potential (p. 340), and to adopt a mode of theorizing based not on what is probable but on what is possible. Indeed, in promoting this view, Hirschman sets up an ideal rule "that anyone who has discovered a new obstacle to development [should also have] the obligation to look for ways in which this obstacle can be overcome" (p. 340). And while this advice originates from epistemological considerations, the argument assumes an ethical character when we are told to refrain from "gloomy visions," not merely because our visions may be highly unreliable, but because they have the tendency to create only "more gloom," to engender a "low self-conception" in the subjects and, irrespective of our intentions, may thereby create a "failure complex" (p. 337) among them.

These arguments have been around for some time. But they have taken on renewed salience in recent years, with the revival of radical subjectivism in social studies and the postmodern critique of the empirical foundations of the larger discipline. At the same time, the case for seeking scenarios of the possible has acquired considerable influence in social thought, especially in studies of political democratization and development. It is thus as much with these trends in mind, as with Hirschman's utilitarian skepticism, that this article revisits the themes. In doing so, the main body of the essay will address the utility of the paradigm concept by retracing particular constructs of macro-, or comparative, politics. Its concluding section, then, will find room for a discussion of the relative merits of probabilistic and "possibilistic" thinking in formulating social theory.

The Paradigm Concept

While the idea of a paradigm—enshrined in the German terms *Denkstil* (thought style) and *Denkkollektiv* (thought collective)—seems to have originated in Ludwik Fleck's study of the genesis and development of scientific facts in the interwar period,[2] and while the current, and somewhat idiosyncratic terminology was already known in the

[2] Fleck, *Entstehung und Entwicklung einer wissenschaftlichen Tatsache: Einführung in die Lehre vom Denkstil und Denkkollektiv* (Genesis and development of a scientific fact: Introduction to the study of thought styles and thought collectives) (Basel: Benno Schwabe, 1935). See also *Genesis and Development of a Scientific Fact*, trans. F. Bradley and T. J. Trenn (Chicago: University of Chicago Press, 1979).

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1950s,[3] both the concept and the particular nomenclature became widely popularized by Thomas S. Kuhn's celebrated volume, first published in 1962.[4]

True, in neither this famous volume nor subsequent elaborations[5] did Kuhn provide crisp and clear definitions; even sympathetic critics found no fewer than twenty-one ways in which the author had used his terminology.[6] But a careful reading of the text enables us to distill three of the most crucial elements of the concept. (1) A paradigm is an intellectual construct with deep social implications in that it both originates in human collectives and also potentially can have an impact on their behavior. (2) As an intellectual construct, it contains an "exemplar," or general proposition, that a relationship exists between two or more analytic categories (such a mass and velocity or economics and politics). And (3) it also contains a number of general, not directly verifiable assumptions about the nature of a larger universe of phenomena—assumptions that provide the logic for the exemplar itself. While often seen as inherently grandiose, like theories, these pretheoretical constructs may appear at different levels of generality, the choice of which in the last analysis will depend on our intellectual curiosities.

Kuhn's arguments about such paradigms are well known and can be summarized briefly. The principal intellectual function of such paradigms is that they enable routine, or "normal," research by designating a generalized locus of phenomena for scholars wishing either to understand a particular fact or to formulate theory by pinpointing variations in the more general scheme. Its social functions are to help maintain the solidarity of scholarly collectives and to provide an organizational focus for their existence. The paradigm dilemma arises out of the limitations of human understanding of the universe. Thus at any particular time new facts or phenomena may arise by discovery, by serendipity, or, in human affairs, by the flow of history, which may be at odds with assumptions deeply entrenched in the discipline. Even, so, as both Kuhn[7] and Fleck[8] predict, the practitioners of the discipline—buttressed by

[3] See, for instance, Robert K. Merton, *Social Theory and Social Structure*, 2d ed. (Glencoe, Ill.: Free Press, 1957), 13–16, 500–555, 582.

[4] Kuhn, *The Structure of Scientific Revolutions* (Chicago: University of Chicago Press, 1st ed. 1962, 2d ed. 1970).

[5] Kuhn (fn. 4, 1970); and idem, "Second Thoughts on Paradigms," in Frederick Suppe, ed., *The Structure of Scientific Theories* (Urbana: University of Illinois Press, 1974).

[6] See Margaret Masterman, "The Nature of a Paradigm," in Imre Lakatos and Alan Musgrave, eds., *Criticism and the Growth of Knowledge* (Cambridge: Cambridge University Press, 1970). See also Douglas L. Eckberg and Lester Hill, "The Paradigm Concept and Sociology," in Gary Gutting, ed., *Paradigms and Revolutions* (Notre Dame, Ind.: University of Notre Dame Press, 1980), 118.

[7] Kuhn (fn. 4, 1962), 77—90.

[8] Fleck (fn. 2, 1935), 35.

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habits of thought, deference to authority, or naked self-interest—at first refuse to accept the occurrence as a counterinstance and prefer to treat it as a variation on a familiar theme. At this point we have the beginnings of a paradigm crisis, for as these counterinstances multiply, they create a sense of anomaly with sociological dimensions that may entail professional demoralization, defection, and, in some cases, even the extinction of the discipline. It is also possible, however, that the crisis is resolved by a breakthrough that goes beyond routinized research, by rejecting old assumptions and fundamentally reconceptualizing the larger universe. The deeper implications of the scheme—which raised Kuhn to the status of a hero scientist—are that the development of knowledge is not cumulative but involves a series of new beginnings, and that these beginnings themselves occur not in a patterned fashion but in a random and unpredictable way.

What does not fit neatly into the schemes of Kuhn or Fleck is that, at the highest level of abstraction, the social sciences have not one but two competing paradigms attracting recurrent attempts at synthesis by numerous scholars including Weber, Parsons, and Habermas. Although the practitioners of social science often shy away from so referring to them, these paradigms have been traditionally designated under the "materialist" and "idealist" labels. Both of these paradigms address the nature and the dynamics of man-to-man, or social, relationships, they are based on different underlying assumptions about human motivation, and they rest on different explanatory exemplars of social change. The master assumption of the first paradigm is that humanity is guided, first and foremost, by its material needs; that the prevalence of material scarcities requires a Promethean effort to control the forces of our object environment. Following these assumptions, man-to-man relations are best explained within the changing context of man-to-nature relations, more specifically, by the changing capacity of humans to wrest from nature the means to satisfy their material needs. By contrast, the fundamental assumption of idealism is that humans are, or may be under specified conditions, motivated by "ideal interests" (Weber) that take precedence over their interest in mere material well-being and survival. So motivated, they are more interested in understanding themselves than in understanding nature. Hence man-to-man relations involve a process of self-discovery, reflected in evolving forms of human consciousness. While this view contains a strong voluntaristic element, the ideas evolved over time constitute a structure of confining conditions that in turn makes social relations amenable to aprioristic reasoning and subject to systematic inquiry.

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Paradigm Shift: The Changing Concepts of System

From Prometheus to Productionism

The social science of the early modern age arose between the fifteenth and nineteenth centuries. In the intervening period the Renaissance gave us the notion that the sources of social change were ultimately in the motivation of humans; the Copernican revolution turned Aristotle's polis into a social system; and the agricultural and industrial revolutions of England focused attention on the means and modes of production. Promethean man struggling with the forces of nature now became *homo oeconomicus*. From these elements there emerged the productionist paradigm associated with the great classics of political economy—the Scottish moral philosophers and Marxian economic theorists of history. Both of these schools of thought start from the same fundamental premise and propose the same three-tiered model of relationships: when human "dexterity" (Smith) or the "means of production" (Marx) change, they change the social division of wealth and labor, which, in the final analysis, compels changes in the structure and exercise of public authority. Conversely, the logic of the paradigm suggests that when politics is changing, the logical sources of change are in the domains of *techne* and economics.

The differences between the liberal and Marxist schools are just as well known as are these points in common. While Marxism teaches that ideal interests are mere reflections of the rational calculation of material gain—or man-to-nature relations—Smith and his Scottish companions recognize the existence of "virtues"—of "fellow feeling" and "self-command"—that coexist with material interests. Coming from outside the division of labor, these forms of consciousness anticipate the modern concept of "culture." It is from this distinction, then, that there developed two theories of progress—in contemporary terminology democratization and development—one unconditional, the other contingent on particular configurations of collective consciousness.

From the Social to the Global System

Whether in its Smithian liberal or Marxist radical form, this productionist paradigm had an unprecedented impact on social thinking, and for a good while remained what Kuhn would describe as the dominant paradigm of political change. There were good reasons for this. The theories that this paradigm spawned are parsimonious and have firm logical underpinnings. They point to a locus of change, a set of intervening variables, and a dependent variable in the structure and organi-

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zation of power. Empirically, the paradigm created convenient shorthands for the jumbled and confusing history of the early modern age. It is not surprising, therefore, that generations of social scientists could not resist the temptation of turning these shorthands into universal laws of human history.

This paradigm of politics, however, ran into a mass of contradictions once transported outside the narrow confines of the early modern Occident. This happened first when Russians and East Europeans began to look at themselves at the turn of the century and then again when American comparativists began to study non-Western societies in the 1950s. Political change seemed to be ubiquitous in both instances, but in neither one were political tensions easily attributable to advances in the system of production. To the contrary, they were often the result of the *absence* of technological progress and economic change.

Though these two situations were separated by more than half a century, the responses of the respective intellectual communities conformed closely to Kuhn's model of a paradigm crisis. In both Russia (in the early 1900s) and in the United States (in the 1950s and 1960s), the initial response to anomaly was to stretch the facts to fit the paradigm, specifically by trying to demonstrate that the economies analyzed were actually experiencing agricultural and industrial revolutions.[9] The next step was to insist that the new reality merely reflected the "telescoping," or "sequencing," of familiar phenomena.[10] Only then did there emerge groups of innovative intellectuals who, impatient with the search for multiple roads and variations in sequences, concluded that the correct interpretation of the subject required not new theory but new categories and causal relationships. These innovators still accepted the relevance of technological breakthroughs as major forces in political change, but they rejected the premise that societies were relatively closed, "natural systems."[11] They advocated instead replacing the nar-

[9] A classic example of such stretching is Lenin's first major work, *The Development of Capitalism in Russia* (1898), 2d English ed. (Moscow: International Publishers, 1964). As for the United States, a search of the catalog of the University of California has yielded seventy-six library entries addressing the political problems of "transitional societies" published between 1958 and 1978. For representative titles, see David E. Apter, *The Gold Coast in Transition* (Princeton: Princeton University Press, 1955); or Bert Hoselitz, ed., *Agrarian Societies in Transition* (Philadelphia: American Academy of Political and Social Science, 1956).

[10] For the notion of "telescoping," see especially Lenin, "Two Tactics of Social Democracy in the Democratic Revolution," in V. I. Lenin, *Selected Works* (Moscow: Progress Publishers, 1970), 1:462–541. For more recent, American examples, see Barrington Moore, Jr., *Social Origins of Dictatorship and Democracy* (Boston: Beacon, 1964); Lucian W. Pye, *Aspects of Political Development* (Boston: Little Brown, 1966), esp. 31–70.

[11] Reinhard Bendix, "Tradition and Modernity Reconsidered," *Comparative Studies in Society and History* 9 (April 1967), reprinted in Bendix, *Nation-Building and Citizenship*, 2d ed. (Berkeley: University of California Press, 1974), 382.

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rower concept of the social division of labor with a broader concept of the global community—a "world system"—divided into central (or "core") and peripheral sectors.[12] Once this intellectual bridge was crossed, it soon became obvious to the scholarly community that there was not a single, universal path, but that there were two distinct patterns of political change: one driven by the socioeconomic consequences of successful technological innovation, the other by the need to overcome the debilitating consequences of economic inferiority, produced and reproduced by the economic success of the core sector.

The Politics of Globality: From Paradigm Shift to Normal Science

The importance of paradigms is that they allow, indeed compel, us to raise new questions. When these questions are answered by empirical research and when these answers are embedded in analogy, we can speak of new theories. In our case, the paradigm shift from the narrower concept of the social to the broader international system has suggested three sets of questions. (1) Why does international inequality so consistently act as confining condition, or "force field," from which the elites of economically inferior nations attempt to break out? (2) What are the strategies available to these elites for accomplishing their project and experiencing upward mobility in the international community? And finally, (3) what is the probability of their success and what conditions lend themselves to positive outcomes?

The Dynamics of Inequality

Perusing the literature on the subject of motivation, we find three sets of hypothetical approaches. First is that the modern state in the West, with its sophisticated institutions and abundant resources, sets new terms for international interaction, competition, and exchange, the costs of which are out of proportion to the meager financial resources of the peripheral economies. Nevertheless, peripheral elites feel the pressure to emulate, for otherwise a nation will be powerless, unable to par-

[12] Immanuel Wallerstein, *The Modern World System* (New York: Academic Publishers, 1974). The classical formula was developed by Alexander I. Helphand (Parvus), Leon Trotsky, and Vladimir I. Lenin. See Winfried Scharlau, "Parvus-Helphand als Theoretiker in der deutschen Sozialdemokratie und seine Rolle in der ersten russischen Revolution" (Parvus-Helphand as the theorist of German social democracy and his role in the first Russian revolution) (Ph.D. diss., Münster, 1964); Leon Trotsky, *Results and Prospects* (1908), published in English as *Permanent Revolution and Results and Prospects* (New York: Merit, 1969); V. I. Lenin, "Imperialism, the Highest Stage of Capitalism" (1916), in *Selected Works*, vol. 1 (Moscow: Progress Publishers, 1970).

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ticipate in global civilization, "and culturally incapable of speaking the international language."[13] As a corollary, a positive response is also encouraged by the fact that building a modern state apparatus provides employment opportunities to economically marginal but upwardly mobile classes. The response, however, will create a vast disproportion between the needs of a modern administration, diplomatic service, and military establishment, on the one hand, and the realistic capabilities of underdeveloped economies to produce necessary surplus and to handle the endemic tension between revenues and expenditures, on the other. Mainline modernization theorists observe that these tensions result from a "reversal" of historical, that is, Western, "sequencing" of state building and economic development.[14]

The second set of explanations arises out of the general critique of the Ricardian theory of comparative advantage, a critique that questions its applicability to the economies of the peripheries. One school, associated with Marxism, locates the problem in processes of exchange and is informed by three interrelated but controversial assumptions: (1) that trade is the key factor in the accumulation of capital and national wealth; (2) that the typical export profile of a peripheral economy consists of "lower ranking" goods including large amounts of human (as opposed to machine) labor; and (3) that when these lower-ranking goods are exchanged for higher-ranking goods, surplus is transferred from the labor-intensive to the capital-intensive economy.[15] Another school shares the Marxist view of the intrinsic disadvantages of backwardness, though its broader and more conventional view of market forces takes them beyond the mechanics of exchange. These disadvantages include differences in income elasticities of demand for the products of the core and the peripheries, and unequal rates of return and remuneration in different sectors of the world economy that result in a steady flow of capital and highly skilled labor from the high-risk, low-wage economies of the peripheries toward the more secure, remunerative environment of the core economies.[16]

[13] Robert W. Tucker, *The Inequality of Nations* (New York: Basic Books, 1977), 63.

[14] Most explicitly in Pye (fn. 10), 31–70. Also in Samuel P. Huntington, "The Change to Change: Modernization, Development and Politics," *Comparative Politics* 3 (April 1971).

[15] See, for example, André Gunder Frank, "The Development of Underdevelopment," and "Economic Dependence, Class Structure, and Underdevelopment Policy," in James D. Cockcroft, André Gunder Frank, and Dale J. Johnson, eds., *Dependence and Underdevelopment in Latin America* (New York: Doubleday, 1972).

[16] See, for example, Gunnar Myrdal, *Rich Lands and Poor* (New York: Harper, 1958), esp. 23–38; Albert O. Hirschman, *The Strategy of Economic Development* (New Haven: Yale University Press, 1962), esp. 183–98; and idem, "The Rise and Decline of Development Economics," in *Essays in Trespassing: Economics to Politics and Beyond* (Cambridge: Cambridge University Press, 1981), 9.

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A third, and perhaps the weightiest, answer to the question of motivation arises from psychocultural factors subsumed under the label of the "international demonstration effect" (IDE). This effect is generated by the material progress of the "pioneering" core societies: while their standards of consumption, comfort, health, and life expectancy are the products of ongoing technological revolutions within national boundaries, the images of these standards travel fast, creating new expectations that the mode of production of economically backward societies cannot fulfill. In the short term the net effect of these expectations is an increase in the propensity to consume and a decrease in the propensity to save. In the longer run, what results is a sense of relative deprivation and social discontent, frequently aggravated by the "paradox of the peripheral state": while the relativization of scarcities requires strong states, such states on the peripheries readily become instruments in the hands of a political class desirous of raising its own material conditions to the standards of the core elites.[17]

The terms *demonstration effect* and *relative deprivation* are of recent vintage. But the ideas underlying them go back a long way in intellectual history, to David Hume in the seventeenth century, Adam Smith in the eighteenth, and Karl Marx in the nineteenth.[18] The insights of these early political economists were further developed in this century by Thorstein Veblen, who pointed out that the economic expectations of the individual are shaped by the consumption patterns of others or, in Veblen's words, by the "force of emulative imitation . . . of standards of reputable expenditure."[19] Significantly, Veblen also observes that "falling short of these" will produce a painful experience of deprivation of a deeper, "spiritual nature."[20] These hypotheses have been upheld by a still more recent generation of economists who have demonstrated empirically that neither consumption nor saving is shaped solely by current income; rather, both are products of the behavior of others or of the individual's past experiences.[21]

[17] See Jean-Philippe Platteau, "Das Paradoxon des Staates in wirtschaftlich rückständigen Ländern" (The paradox of the state in economically underdeveloped countries), *Oesterreichische Zeitschrift für Soziologie* 9, no. 4 (1984).

[18] For a summary of British thinking on the relativity of scarcity, see Nicholas Xenos, *Scarcity and Modernity* (New York: Routledge, 1987), 12–21; and David Hume, "Of Commerce," in Eugene Rotwine, ed., *Writings in Economics* (Madison: University of Wisconsin Press, 1970), 14. For Marx, see Karl Marx, "Wage Labor and Capital," in Robert C. Tucker, ed., *Marx-Engels Reader* (New York: W. W. Norton, 1972), 180.

[19] Veblen, *Imperial Germany and the Industrial Revolution*, 3d ed. (New York: Vintage, 1954), 147, 208.

[20] Ibid., 208.

[21] See James S. Duesenberry, *Income, Saving and the Theory of Consumer Behavior*, 5th ed. (Cambridge: Harvard University Press, 1967), 27; Harvey Leibenstein, "Bandwagon, Snob and Veblen Effects in the Theory of Consumer Demand," *Quarterly Journal of Economics* 64, no. 2 (1950).

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But these empirical models, however carefully constructed, still leave us with the question of why the demonstrated welfare of a few should turn into "desire," expectation, and frustration on the part of the many who fail to attain it. This question is especially justified in view of the fact that prior to the seventeenth and eighteenth centuries, differentials of wealth among societies, though widely noticed, did not have the same unsettling consequences as they have in the modern age. There are two possible solutions to this puzzle. Either one may point to the special quality of changes that followed in the wake of the technological revolutions of the modern age; these brought about not merely incremental increases in human comfort but also a profoundly new calculus in man-to-nature relations as human control over time, space, health, and life expectancy increased. Or one may argue, together with Ted Gurr, that new expectations arise not merely in response to new images of material life but also in the context of new forms of social consciousness,[22] as part of what Karl Mannheim described as the "fundamental democratization"[23] of the world brought about by the diffusion of the universalistic and egalitarian norms of the original core countries. It is in this context that people at different stations in life begin to see themselves as being "in the same boat"[24] with those more prosperous than they and hence as entitled to the same material benefits. Those who fail to receive these entitlements experience a sense of injustice, the "spirituality" of the experience that Veblen once observed.

The relevance, or even existence, of the international demonstration effect has been disputed by some,[25] and it is either ignored by neo-Marxist scholarship or dismissed as a hoax, notwithstanding Marx's own affirmation of the importance of relative "immiseration."[26] Others, like Daniel Lerner, Karl de Schweinitz, or Reinhard Bendix,[27] have made good use of the concept, and to this writer, it appears to be the key to understanding the politics of the peripheries in the modern world. To be sure, the concept needs further refinement, by establishing typical variations in its intensity together with explanations of these

[22] Gurr, *Why Men Rebel?* (Princeton: Princeton University Press, 1970), 101.

[23] Mannheim, *Men and Society in an Age of Reconstruction*, trans. E. Shils (New York: Harcourt, Brace and World, 1940), 43–44.

[24] Walter G. Runciman, *Relative Deprivation and Social Justice* (London: Routledge and Kegan Paul, 1966), 9.

[25] Anthony Oberschall, "Rising Expectations and Political Turmoil," *Journal of Development Studies* 6 (October 1969).

[26] See, for example, André Gunder Frank, "The Sociology of Development and the Underdevelopment of Sociology," in Cockcroft, Frank, and Johnson (fn. 15).

[27] Lerner, *The Passing of Traditional Society* (Glencoe, Ill.: Free Press, 1969); Karl de Schweinitz, *Industrialization and Democracy* (Glencoe, Ill.: Free Press, 1964), 234–69; Reinhard Bendix, *Kings and Peoples* (Berkeley: University of California Press, 1978), 12–13.

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variations in terms of cultural and geographical distance from the reference societies of the West. But even without these refinements the concept points to the fundamental relativity not only of deprivation and scarcity but also of social structures and processes. While the old sociology taught that these could be plucked out of any historical context and be used to draw identical conclusions about politics in any other historical period, the new sociology of globality teaches that the political meanings of structure and process are bound to change over time. Certainly, the same quantity of industrialization, literacy, social mobilization, and, most obviously, per capita income had different implications for politics in eighteenth-century Britain, nineteenth-century Russia, and twentieth-century China or India.

Strategies of International Mobility

What, then, is the proper and likely response of peripheral elites to these confining conditions? A survey of the intellectual landscape of European sociology yields two general answers. One is that the economically inferior nation should marshal its internal resources and develop its means of production, so as to achieve self-sustained growth, high levels of domestic consumption, and competitiveness in the global markets. The other is that the nation should engage in "external appropriation," a euphemism for plunder and territorial conquest. Both strategies hinge on the state, for its power is needed to mobilize society for either of the collective purposes. If and when these strategies become the supreme rationale for organizing and legitimating the political system, we can speak of "developmental" and "militarized" states, respectively.

While both of these strategies of international mobility have long academic pedigrees, American students of comparative politics have always shown a strong bias in favor of developmental studies. True, after the Second World War there was a flurry of excitement and a wave of anguish among comparativists about a world populated by "garrison states" obsessed with military security.[28] But, ironically, by the time the cold war broke out, development had been firmly ensconced as a master concept of comparative politics, with the field of international relations monopolizing the study of force and war making. The consequences of this division of labor were often debilitating for the study of domestic structures of authority. Not only were many stagnating societies of the

[28] Stimulated largely by Harold Lasswell, *The Analysis of Political Behavior* (London: Kegan Paul, 1947), 153–55.

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Third World seen as actually "developing," but the developmental frame of reference was also imposed on the study of a number of revolutionary regimes with obvious external ambitions and geopolitical objectives. This was especially true in the case of Soviet studies, where a number of social scientists and historians were willing to see the Stalinist model of industrialization as an efficient, if overly rough, shortcut to a modern society.[29] Although the developmental label was reserved almost exclusively for left-wing regimes,[30] even the regimes of the right were sometimes presented in the developmental frame of reference, as embodiments of antimodernism, anti-industrialism, and antidevelopmentalism[31] or as the products of a perverse, inverted path (*Sonderweg*) of political development on the road to industrialism.[32] For those uncomfortable with the developmental framework, the escape route was either via the totalitarian hunger for power or via theories of stress, marginalization, and cultural despair.[33]

The idea of a developmental state goes back to French mercantilism in the eighteenth century and to German political economy in the nineteenth century.[34] In the United States, however, it only struck root after the Second World War, when the country entered the world scene as a superpower with a geopolitical agenda of its own. Once established in America, the study of development went through two stages. In the

[29] See, among others, Issac Deutscher, *Russia, What Next?* (New York: Oxford University Press, 1953); Theodore von Laue, *Why Lenin, Why Stalin?* 2d ed. (Philadelphia: Lippincott, 1971); J. Kautsky, *Political Change in Underdeveloped Countries* (New York: Wiley, 1962); Alec Nove, "The Soviet Model and Under-Developed Countries," *International Affairs* 37 (January 1961); Cyril Black et al., *The Modernization of Japan and Russia* (New York: Free Press, 1975); and Oleg Hoettding, "The Soviet Union: Model for Asia, State Planning and Forced Industrialization," *Problems of Communism* 8 November–December 1959). For more, see T. Anthony Jones, "Modernization Theory and Socialist Development," in Mark Field, *Social Consequences of Modernization of Communist Societies* (Baltimore: Johns Hopkins University Press, 1976).

[30] A. James Gregor, *Italian Fascism and Developmental Dictatorship* (Princeton: Princeton University Press, 1979).

[31] For Germany, see George L. Mosse, *The Crisis of German Ideology: Intellectual Origins of the Third Reich* (New York: Schocken, 1964); Armin Mohler, *Die Konservative Revolution in Deutschland* (Darmstadt: Wissenschaftliche Buchgesellschaft, 1972); Klemens von Klemperer, *Germany's New Conservatism* (Princeton: Princeton University Press, 1957); see also the discussion of the "totalitarianism of the aristocracy" in Kautsky (fn. 29), 97–106.

[32] Moore (fn. 10); Ralph Dahrendorf, *Society and Democracy in Germany* (New York: W. W. Norton, 1967), esp. chap. 25.

[33] Hannah Arendt, *Origins of Totalitarianism* (New York: Harcourt Brace, 1951); Norman Cohn, *The Pursuit of the Millennium* (New York: Oxford University Press, 1960), xii–xvi, 307–20; J. L. Talmon, *The Origins of Totalitarian Democracy*, 2d ed. (New York: Praeger, 1960); Michael Walzer, "Puritanism as Revolutionary Ideology," in S. N. Eisenstadt, ed., *The Protestant Ethic and Modernization* (New York: Basic Books, 1967); Fritz Stern, *The Politics of Cultural Despair* (Berkeley: University of California Press, 1961).

[34] Friedrich List, *The National System of Political Economy* (1841), trans. Sampson S. Lloyd (London: Longman's, 1916); Johann-Gottlieb Fichte, "Der geschlossene Handelsstaat" (The closed trading state), in Fichte, *Ausgewählte Werke*, vol. 3 (Darmstadt: Wissenschaftliche Buchhandlung, 1962).

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first, under the unmistakable, if unacknowledged, influence of Marxism, the ventures in theorizing focused on the accumulation of capital. This was reflected in what Hirschman calls a "competition of metaphors" among the concepts of "takeoff" (Rostow), "big push" (Rosenstein-Rodan), "great spurt" (Gerschenkron), "critical effort" (Leibenstein), and "hump" (deSchweinitz), all obstacles to be overcome so that the economy could reach a plateau of "self-sustained" growth, or "normal economics.[35] In the second stage, inspired largely by the experience of the newly industrialized countries of the Pacific Rim, the focus of inquiry shifted from coercion to coordination and to the "soft authoritarianism" of the capitalist developmental state, the chief functions of which were seen in relaying market signals from the world economy and in absorbing some of the risks of foreign export ventures.[36]

The elaboration of the military option as an instrument of international mobility was the work of a long line of German historians and sociologists, from Leopold Ranke in the nineteenth century to Otto Hintze in the twentieth century. In one way or another, all of these scholars were associated with the school of the "primacy of external politics" (*Primat der auswärtigen Politik*). Whether as political advocacy or empirical sociology, this school juxtaposed trading with war, and economic production with "wringing out" (*ausstreben*) the means of subsistence from the external environment.[37] Ironically, though, while German scholarship led the way in the study of "externally oriented activities," perhaps the most succinct statement on the topic came from an Englishman, Herbert Spencer, toward the end of the nineteenth century. Much like the other great "productionists" of his century, Spencer, too, saw technology as the main driving force of social change.

[35] Hirschman (fn. 16, 1981), 10.

[36] For the original formula of the capitalist development state, see Chalmers A. Johnson, *MITI and the Japanese Miracle: The Growth of Industrial Policy, 1925–75* (Stanford, Calif: Stanford University Press, 1982). See also idem, "Political Institutions and Economic Performance: The Government-Business Relationship in Japan, South Korea and Taiwan," in Frederic C. Deyo, ed., *The Political Economy of the New Asian Industrialism* (Ithaca, N.Y.: Cornell University Press, 1987); idem, "The Institutional Foundations of Japanese Industrial Policy," *California Management Review* 23 (Summer 1985); Youngil Lim, *Government Policy and Private Enterprise: The Korean Experience in Industrialization* (Berkeley: Institute of East Asian Studies, University of California, 1981); and Stephan Haggard, *Pathways from the Periphery* (Ithaca, N.Y.: Cornell University Press, 1990).

[37] Leopold Ranke, *Die grossen Mächte: Politisches Gespräch* (The great powers: Political discourse) (1836; Göttingen: Vandenhoek und Rupprecht, 1962), esp. 60–61; Hans Delbrück, *Geschichte der Kriegskunst im Rahmen der politischen Geschichte* (History of the art of war in the context of political history), 3d ed. (Berlin: deGruyter, 1962); Otto Hintze, *Historical Essays*, ed. and trans. Felix Gilbert (New York: Oxford University Press, 1975), esp. 157–78. For an excellent survey of this literature, see Ernst-Otto Czempiel, "Der Primat der auswärtigen Politik: Kritische Würdigung einer Staatsmaxime" (The primacy of external politics: Critical discussion of a principle of statecraft), *Politische Vierteljahresschrift* 4 (September 1963).

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But unlike Marx and Smith before him, he drew a clear line between the technologies of production and destruction and recognized that challenges of change for societies are posed not only by nature but also by other, competing societies. Subsequently, it was on these conceptual distinctions that Spencer built his famous dichotomy between "industrial" and "military" societies,[38] the one organized to pursue such "internal activities" as "agriculture, manufacture and trading," the other to plunder and to conquer.[39] To be sure, Spencer was well aware that, especially in modern times, the success of military activities required manufacturing industries and an elaborate infrastructure. But he also tells us that in the case of a military society this "industrial part continues to be essentially a "permanent commissariat, existing solely to supply the needs of governmental military structures, having left over for itself enough merely for bare maintenance."[40] Hence, Spencer added perceptively and with great foresight, its activities would be closely supervised and coordinated by political authorities. More clearly than Smith and Marx before him, he recognized the distinction between means of production and means of destruction, between producing for the market and producing for war. Unlike more recent students of industrial society, including most theorists of "convergence,"[41] Spencer clearly understood that the same term, "industrialization," could actually be applied to two very different processes. In one case, it refers to the rise of a military-industrial complex based on the technologies of destruction, a process led *by the state for the state*; in the other it refers to processes that lead to increasing popular welfare and international competitiveness. This duality of meanings should allow us to distinguish, say, the industrial revolution of England between 1780 and 1830 from the Stalinist "industrialization" of Russia in the 1930s and 1950s, so often conflated by industrial sociology into a single process with converging political outcomes.

Albeit more than a hundred years old, Spencer's dichotomy may provide a useful tool for social science and a much needed corrective for the heavy developmental bias of comparative politics. Some amplification of the scheme may be in order, however. For one, we must recognize that in modern times, states may set out to plunder and conquer

[38] Herbert Spencer, *On Evolution*, ed. J. D. Y. Peel (Chicago, Ill.: University of Chicago Press, 1972).

[39] Ibid., 152.

[40] Ibid., 154.

[41] For a major statement on this subject, see Clark Kerr et al., *Industrialization and Industrial Man* (Cambridge: Harvard University Press, 1966). See also Alfred Meyer, "Theories of Convergence," in Chalmers Johnson, ed., *Change in Communist Systems* (Stanford, Calif.: Stanford University Press, 1970).

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territory not only as ends in themselves but also as part of a larger scheme for "reconstructing"[42] the international environment. This recognition provides a concept with which to rescue the study of national socialism, fascism, and communism both from the developmental frame of reference and from the various theories emphasizing social pathologies. This, in turn, facilitates our drawing further distinctions between militarized societies and movements, not just in terms of their "systemic" and "antisystemic" (or anti–status quo) objectives,[43] but also in terms of their "totalitarian" and "incrementalist" objectives. While the former may apply to movements and regimes that set out to change the very rules and terms of resource allocation and exchange, the latter may refer to designs for changing the existing balance of power in a given region or in the world. The prime examples of totalitarianism in the modern world have been communism and national socialism: while one hoped to replace the market with a global community of perfect equality, the other promised to build a world order of perfect inequality in which the naturally superior would command and the inferior would obey. In contrast, Wilhelmine imperialism and Italian fascism serve as examples of ideological incrementalism. While Mussolini wanted to turn the Mediterranean into an "Italian lake" and restore the glories of ancient Rome, he rejected, in his own words, the "teleological notion that at some future time the human family will secure final settlement of all its difficulties."[44]

This project of "bringing war back" into comparative politics will also invite the discipline to formulate theories of the dynamics of militarism, especially when used as an instrument of international mobility. Here German theorizing, from Clausewitz to Hintze,[45] and the history of Prussia may provide a helping hand. Indeed, since in the eighteenth century Prussia was an economically marginal state on the periphery of the European continent, its experience may be a good model of how an economically disadvantaged state can fight its way "up" from the periphery to the higher echelons of the modern world economy.

The model that emerges is both linear and cyclical. Its linearity derives from Clausewitzian military doctrine, which teaches that one

[42] This term has been chosen in deference to Karl Mannheim (fn. 23), who used it to describe the essence of modern ideology.

[43] For this distinction, see Immanuel Wallerstein, *The Politics of the World Economy* (New York: Cambridge University Press, 1984), 97–111. See also Giovanni Arrighi, Terence K. Hopkins, and Immanuel Wallerstein, *Anti-Systemic Movements* (New York: Verso, 1989).

[44] Benito Mussolini, *Fascism: Doctrine and Institutions* (Rome: Ardita, 1935), 10.

[45] Karl von Clausewitz, *Vom Kriege* (1832). English translations abound. References in this article are from *The Living Thoughts of Clausewitz*, trans. and ed. Joseph I. Greene (New York: Longmans and Green, 1943); for the other authors, see fn. 31.

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should chose one's final target carefully, avoid nibbling around the periphery, and always strike at the main force of the enemy. But each state is a cycle that begins with the mobilization of the resources of a relatively poor society and culminates in war. If successful, war leads to the acquisition of new resources or the conquest of territory with new, preferably more advanced productive capacity. This permits the replenishment of resources and offers a breathing space for the population, before it embarks on a new project of mobilization and austerity. These cycles of mobilization and conquest were followed rather meticulously by all of the great challenger states of the twentieth century: Hitler's Germany (1935–41), Stalin's Russia (1939–40, 1945–48), Tojo's Japan (1935–41), and, more lackadaisically, Mussolini's Italy (1936–40). Of course, as these examples demonstrate, strategy is not victory. The cycle of mobilization and advance may be broken by defeat, as was the case with the Axis powers, or by exhaustion and deterrence, as was the case with the Soviet Union.

Finally, if Spencer's scheme is to be made more workable, it must be stripped of its rigid functionalism. True, in some sense, societies *are* actors engaging each other. But societies are composed of classes with divergent interests, and these class interests often supersede the interests of the whole. In this respect, it is especially important to remember that societies are divided not only into economic classes but also into political classes, the former being the producers of goods, and the latter being the producers and reproducers of society with their own vested interest in the state. With this in mind, one can solve a number of puzzles that the holistic "societal" models cannot solve, above all, why "nations" strive to be independent, even though the economic consequences of independence may turn out to be beneficial only to those who directly profit from controlling the state machine, by being in its employ. Examples of this abound in both the nineteenth and the twentieth century.

More so than functionalism, a class perspective also demonstrates how hollow the concept of "national interest" can become when a well-fed and privileged elite displays "altruism" toward foreign clients even while imposing monumental sacrifices on its own subjects, as the Soviet elite did for so many years. As a corollary it is not functionalism but class theory, amplified by the recognition of the role of individuals (in the mode of Weber), that can appropriately explain what may be described as the "Hitler (or Saddam) syndrome" in which societies are put at risk and sacrificed, not for their own good, but for the "higher interests" of their leaders.

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Explaining Outcomes: Institutions, World Economy, World Politics

The third set of questions raised by the global paradigm concerns specific outcomes and, more broadly, the pattern of international mobility in the modern world. By such mobility is meant not merely a high rate of economic growth, but specifically a rate of growth that puts one society ahead of others, such that it finds itself in the company of the "better placed" nations in the core region of the world economy.[46]

While the absence of historically deep national accounts makes the accurate reconstruction of the past somewhat difficult, there are sufficient numbers of learned estimates that allow us to discern the dominant trends, especially because the conclusions of those individual studies show considerable convergence. One of the most striking and least disputed conclusions is that over the past two centuries the distribution of wealth *among* national societies has followed a diametrically opposite trend from the experience *within* national societies. Thus while income disparities within national entities have been progressively diminishing over the past two centuries, economic disparities among national societies and global economic sectors have been steadily increasing, reaching current levels that would be politically unacceptable in the great majority of contemporary nation-states. We can cite here Paul Bairoch's samples drawn from the First and the Third Worlds. They show that, even if adjusted to purchasing-power parities, per capita incomes in the two sectors have moved from relative equality in 1750–1800 to ever-increasing disparities throughout the nineteenth and twentieth centuries. Specifically, the ratios of per capita incomes between the two sectors moved from near parity in 1800 to 100:77 by 1830; by 1860 it had widened to 100:53; in 1910 the ratio was 100:32; in 1950, 100:19; and in 1977, 100:13.[47] A similar, though somewhat less steep increase in disparities may be observed in comparisons between the U.S. and Latin America, and between Western and Eastern Europe.[48] Other, more complete samples show still greater and more rapidly increasing disparities. A comparison of the top and bottom 25 countries in 1978 and 1993 (from a list of 179) shows that the disparities between them (calculated at exchange rates) have increased

[46] J. P. Nettl and Ronald Robertson, *International Systems and the Modernization of Societies* (London: Faber, 1968), 56–57.

[47] Paul Bairoch, "The Main Trends in National Income Disparities since the Industrial Revolution," in Paul Bairoch and Maurice Lévy-Leboy, *Disparities in Economic Development since the Industrial Revolution* (London: Macmillan, 1981), 7.

[48] Angus Maddison, *Monitoring the World Economy, 1820–1990* (Paris: OECD, 1995), 23–25.

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from 1:50 to 1:88, or from 1:15 to 1:23, if adjusted to purchasing-power parities.[49]

Likewise, mobility from the poor to the middling and from the middling to the rich has been relatively limited. Of course, we know about the rise of English-speaking countries overseas and about the Scandinavian countries closing the gap between themselves and those of the others in Atlantic Europe. But in Angus Maddison's sample of fifty-six countries, nine of the twelve richest in 1820 were still part of the top twelve in 1992.[50] Meanwhile Bairoch's sample of twenty-four European countries shows that eight of the top ten in 1830 were in the same group in 1913, with no further changes between 1913 and 1950, though with a turnover of four countries between 1950 and 1975.[51] On the global scale as well, new, higher degrees of mobility are apparent after 1950, with Japan, Finland, and Spain moving into the circle of the top 25 (of 133 and later 179 countries), together with 7 sparsely populated oil states. But further mobility was more modest. After 1978 Ireland, Hong Kong, and Singapore entered the top cohort, with Libya, Czechoslovakia, and East Germany eventually exiting.[52] In the middle, the so-called NICs were moving upward from among the ranks of the poor, but as of 1995, they had not entered the twenty-five on top.

While instances of mobility of this sort have been more the exception than the rule, not surprisingly it has been the few stories of success that captured the attention of scholars theorizing about outcomes. Most of the resultant theories point to the role of culture or institutional configurations, the latter explanations, as already noted, focusing either on the coercive or on the coordinative functions of the state.[53] The problem with this mode of theorizing is that it tells us only a small part of the story of international mobility—merely pointing to the necessary, and not to the sufficient, conditions of economic success. If transposed to the realm of microeconomics, these theories would tell us only that entrepreneurs need virtue, capital, and information, but they would tell us little about changing supply and demand and other factors external to the particular unit.

One scholar of East Asia, Hagen Koo, makes this last point quite forcefully, if only indirectly, in the context of the experience of the NICs.

[49] Based on World Bank, *World Development Report, 1978* (Washington, D.C.: World Bank, 1978); and idem, *World Development Report, 1995* (New York: Oxford University Press, 1995).

[50] Maddison (fn. 48), 23–24.

[51] Paul Bairoch, "Europe's Gross National Product, 1800–1975," *Journal of European Economic History* 5 (Fall 1976), esp. 286, 297.

[52] World Bank (fn. 49, 1978, 1995).

[53] In addition to the sources in fn. 36, one may cite the articles in a popular collection published and

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As Koo writes: "Although the statist approach reveals important dimensions of East Asian development, it tends to overstress the independent role of the developmental state, paying insufficient attention to . . . the world economic system."[54] It does so by ignoring the fact that the countries of the Pacific Rim

> embarked on export-led industrialization ahead of most other Third World countries. . . . Timing was important. In the 1960's and 1970's the world economy was on the upswing. Core capitalists were eager to invest, Euromoney was easy to borrow, and interest rates were relatively low; there were fewer trade barriers against Third World manufactures and little competition from other labor-rich developing countries.[55]

But Koo makes an even more powerful point when he reminds us that, in the case of the Pacific Rim, as important as the world economy and the cultural-institutional factors of the given societies was the involvement of the United States in regional politics, "mainly to counter Soviet expansion to the Far East after the outbreak of the Korean war."[56] It was in this context that there followed the influx of large amounts of foreign aid and the opening of American markets to imports that in economic terms alone made little sense. Thus, he continues, "when we talk about the roles of South Korea and Taiwan in the world system, we must consider their position not only in world capitalism but [also] their roles in the world's political and military confrontations."[57]

While some East Asian experts may take exception to the particulars of this argument, Koo's general point has profound implications for the discipline. Simply put, there exists an international political system alongside the global market, and neither of these two systems can be reduced to each other in their entirety. One cannot explain politics by economic outcomes or economics by the distribution of power. The point further implies that while states or their political classes seek to improve their status in the world economy, they are also concerned about their security and at times will be willing to pay an economic

republished several times in the 1960s and 1970s. See the following essays in Jason F. Finkle and Richard W. Gable, *Political Development and Social Change* (New York: John Wiley and Sons, 1971): David C. McClelland, "Achievement Motive in Economic Growth"; Robert N. Bellah, "Religious Aspects of Modernization in Turkey and Japan"; Bert F. Hoselitz, "Non-economic factors in Economic Development"; Manning Nash, "Some Social and Cultural Aspects of Economic Development"; and Robert E. Ward, "Political Modernization and Political Culture in Japan."

[54] Hagen Koo, "The Interplay of State, Social Class, and World System in East Asian Development: The Cases of South Korea and Taiwan," in Deyo (fn. 36), 165.

[55] Ibid., 169.

[56] Ibid., 167.

[57] Ibid.

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price for physical or cultural survival. Consequently, when dealing with the problem of international mobility, one should pay heed not only to international competition and exploitation but also to various forms of international cooperation and co-optation. It is true that prior to 1945 instances of co-optation had been relatively rare. But during the cold war period projects of politically motivated resource transfer became a standard weapon in the arsenals of liberal capitalism and state socialism. Indeed, some of the great economic "miracles" of the postwar period—the experience of Japan, Germany, and the other countries of Western Europe—can hardly be explained outside the context of the Marshall Plan, its Far Eastern counterparts, and the broader international regime as based on the earlier Bretton Woods agreements. It was under the latter that the United States established its trading and currency regime, the essence of which was tariff preferences combined with a modified gold standard and a grossly overvalued U.S. dollar. At a different level, these arrangements were replicated by the European Common Market into which the industrial nations of Northwest Europe co-opted first semiperipheral Italy and later peripheral Spain, Portugal, and Greece.

These recent experiences offer us the elements necessary for constructing theory. First, like Adam Smith's butcher and baker, the major actors in these arrangements are motivated not by benevolence but by self-interest. Second, the potentials of these ventures in international income transfer are not unlimited. They are, above all, limited by the resources available to the donor. In 1945 the U.S. had the resources to co-opt and "save" Germany and Japan, but not even at the height of its economic power could it reasonably hope to co-opt the Soviet Union and China, a simple fact that all parties gradually realized and that may well be the best explanation of how and why the cold war came about.

Remarkably, with very few exceptions,[58] the initiative for studying the impact of external political constraints on domestic outcomes has come not from comparative politics but from the field of international relations, most notably from hegemonic theory.[59] At the heart of these

[58] Most prominently, Perry Anderson, *Lineages of the Absolutist State* (London: New Left Books, 1974); Theda Skocpol, *States and Social Revolutions* (London and New York: Cambridge University Press, 1979).

[59] See Peter Gourevitch, "The Second Image Reversed: International Sources of Domestic Politics," *International Organization* 32 (Fall 1978); and G. John Ikenberry and Charles A. Kupchan, "Socialization and Hegemonic Power," *International Organization* 44 (Summer 1990). See also Robert O. Keohane, *The Theory of Hegemonic Stability and Changes in International Regimes*, Monograph no. 28 (Los Angeles: UCLA Center for Strategic Studies, 1980).

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theories is the recognition (1) that the countries of the world are divided not only between rich and poor but also between more and less powerful states, and (2) that the former exercise their power not only by acts of war but also by influencing both economic outcomes and the shape of political institutions in weaker states. This simple fact, so central to the German sociology of the *Primat* in the nineteenth century,[60] has all but eluded contemporary theories of democratization and development and has allowed them to acquire their quasi-voluntaristic cast by ignoring the prevalence of structural constraints because they emanate from outside the polity. These lessons of the past are especially relevant if we set out to predict economic and political outcomes in postcommunist societies, none of which float in an external political vacuum, but rather are passing from one form of external hegemony to another.

To be sure, we must be mindful of the fact that hegemonic power, however potent, inevitably runs up against the resilience of domestic structures and cultures, which will bend under its weight but will not disappear. In the process, the hegemon's designs may not be copied but will be simulated and "corrupted." This is important to remember when thinking about the prospects of "new democracies" sponsored by liberal hegemons like the United States and the European Union. The true challenge for comparative analysis therefore is not to bring back the old precepts of the *Primat* of external politics but to strike an appropriate balance between the external and the internal and, not least of all, between the forces of external markets and external politics.

Paradigms of Postmodernity

Epistemological Dilemmas of Postmodernity

To take stock, by the end of the 1970s social science had a new paradigm capable of integrating elements of international relations into comparative politics. In providing a new framework for the study of political change, this paradigm enables us to identify new patterns and conditions of international mobility and opens up new vistas for the study of the radical left and right beyond the classical models of totalitarianism. Overall, the paradigm provides a new, parsimonious formula for understanding the dynamics of modern history—as a change in man-to-nature relations in a few societies of the world that has bene-

[60] See fn. 37.

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fited some of humanity while also creating new sources of conflict leading to great wars, both hot and cold.

For the social scientist, of course, the question is not merely whether one can find the proper keys to history through a parsimonious formula that allows us to fit the pieces of an untidy past into the puzzle. It is also whether and to what extent the past can be used to predict the future or, more modestly put, to engage in meaningful discussion about it. To do so, we must be able to assume that certain aspects of the past can be held constant. If we cannot make this assumption, we must be prepared for another paradigm crisis and cycle of change.

Even as it is expected in our Kuhnian universe that history turns constants into variables, the latest challenge to the validity of the existing paradigm of modernity nonetheless came as a shock to most practitioners of political sociology, because the events unfolded where they were least expected—in the advanced industrial societies of the world. Mainstream theories of modernization assumed that progress and prosperity immunized societies to political turmoil. And yet, as the 1960s were turning into the 1970s the earlier tranquillity of the "core" societies came to an abrupt end as some of the principal beneficiaries of its wealth turned against existing institutions, often in violent protest. Revolution amidst prosperity was an anomaly with which classical theories of industrial society could not cope. Thus before long the discipline was saddled with another paradigm crisis.

Compared with the earlier instances discussed, this most recent paradigm crisis has a number of distinctive features. Most remarkably, today's crisis has generated the rejection not only of the prevailing paradigm—technological innovation equals social progress equals prosperity equals tranquillity—but also of the very concept of paradigm. If, as Zygmunt Bauman suggests, throughout most of the past two centuries we have been nurturing the belief that "life without paradigm is but a temporary and curable condition," the new, postmodern epistemology tells us that this "hope is false, the belief misguided and misguiding."[61] There are gradations in this rejection, and few of them will be new to those familiar with eighteenth- and nineteenth-century masters of historicism and radical subjectivism or to those who have been following the conversations between social scientists and historians. Some, like Charles Tilly, reject the study of "big structures, large processes and huge comparisons" on the grounds of utility and style. Reading his critique (essentially of the older, more rigid systemic concept), one comes

[61] Bauman, *Intimations of Postmodernity* (London and New York: Routledge, 1992), 68–69.

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away with the idea that engaging in such comparisons, while not absurd, is a waste of time.[62] Others, like Susanne Hoeber Rudolph, are opposed to macro-social projects for their holism, which encourages the intellectual "compulsion to suppress alternative truths."[63] Again others have lost their taste for "metaphors" or "meta-narratives"[64] because, in the fashion of German historicism, they have abandoned faith in the power of analogy, in the power of the human mind to reassemble discrete events into patterns that have explanatory and predictive power because they are likely to repeat themselves. The solutions that these doubters offer are familiar from the work of epistemological pessimists of previous ages who, like Robert Merton, advised us to stay close to the facts, to keep our distance from grand schemes, and to operate in carefully defined empirical and chronological contexts to produce theories of the "middle range."[65]

If, much like Hirschman, the above critics of the paradigm concept object on utilitarian grounds and take their cue essentially from German historicism, rival scholarly endeavor harks back to the phenomenology of eighteenth-century radical subjectivism. This subjectivism teaches us that the world is little more than an image (*Vorstellung*) in the consciousness of observers endowed with little capacity for meaningful exchange of experience. In the eyes of such subjectivists and their modern disciples, like Jacques Derrida[66] or Jean-François Lyotard,[67] paradigms are not the tools of *homo faber*, but of *homo ludens*, the representations of playfully constructed harmonies pretending to link essentially unlinkable networks of impressions. Some of these radical subjectivists reject not only the idea of causality—the traditional sign of a paradigm crisis that we also observed in the 1960s, when the classical notion of system was dying—but even the very possibility of decoding "meanings as products, reflections, aspects or rationalizations of social configurations."[68] Needless to say, within this epistemology the basic premises of comparative and international politics are anomalies, since "the world [is] composed of an infinite number of meaning-generating

[62] See Charles Tilly, *Big Structures, Large Processes, Huge Comparisons* (New York: Russell Sage Foundation, 1984).

[63] Rudolph, contribution to "The Role of Theory in Comparative Politics: A Symposium," *World Politics* 48 (October 1995), 28.

[64] See, for example, Robert A. Nisbet, *Social Change and History* (New York and London: Oxford University Press, 1969), esp. 220–23, 249–50.

[65] Merton (fn. 3), 9, 280, 328.

[66] Derrida, *Margins of Philosophy*, trans. A. Bass (Chicago: University of Chicago Press, 1982).

[67] Lyotard, *The Postmodern Condition: A Report on Knowledge*, trans. Geoff Bennington and Brian Marsuni (Minneapolis: University of Minnesota Press, 1984).

[68] Bauman (fn. 60), 26.

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agencies, all relatively self-sustained and autonomous, all subject to their respective logics and armed with their own facilities of truth validation."[69] All that remains for the political sociologist is to attempt to translate separate and individualized "narratives" from one cultural code to another or to tell narratives of injustice anchored in the self rather than in some historically established standard.

The sympathetic observer of this epistemological argument runs up against the timeless paradox of philosophical skepticism—if no truth can be discerned, how can this particular pronouncement be validated?—and feels compelled to admit that the epistemological critique may be (1) taken at face value, (2) taken as a symptom of a classical paradigm crisis, or (3) seen as the by-product of a larger sociocultural crisis of Western society.[70] This then leads to the recognition that postmodernity refers both to an epistemological critique and to an empirical project to make sense of a new epoch, the key features of which are no longer captured by earlier paradigms of modernity. This empirical project is not without its own ambiguities, for its results so far have produced not one, but two competing paradigms. One of these we may designate as the paradigm of postindustrialism, the other as that of postmaterialism.

Empirical Dilemmas

The postindustrial paradigm is rooted in its precursors in two ways—first, that man-to-man relations are best explained in terms of changing relations between man and nature, and second, that humanity is motivated in the main by the "modern" goal of emancipation from economic scarcity. What has changed is the calculus of technology: the quantum of human control over nature, space, and time seen to be producing qualitative changes in man-to-man relationships.

These assumptions have given rise to at least three sets of progressively more gloomy theories. The first of these is epitomized by the futurologist school of Alvin Toffler, which, while not oblivious to the perils of new technologies, is on balance exuberant about the potentialities of computers and electronics.[71] By contrast, Daniel Bell and Morris Janowitz, who gave us the label of postindustrialism,[72] are more

[69] Ibid., 35.

[70] Ibid., 43.

[71] See, for example, Alvin Toffler, *Future Shock* (New York: Random House, 1970); and idem, *Knowledge, Wealth and Violence at the Edge of the Twenty-first Century* (New York: Bantam, 1990).

[72] Bell, *The Coming of Post-Industrial Society: A Venture in Social Forecasting* (New York: Basic Books, 1973); Janowitz, *The Last Half-Century: Societal Change and Politics in America* (Chicago: University of Chicago Press, 1978).

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reserved in their prognoses. They see the nature of technology changing, which under even the best of circumstances is a signal for turmoil. And while, in the best tradition of Newtonian science, they can envisage the rise of a new equilibrium, they are not certain that it will be compatible with the familiar forms of liberal democracy.

A third elaboration of postindustrialism is still in the making, having thus far been articulated in fragments rather than as a consistent whole.[73] These fragments address different aspects of technological change: (1) the rise of "heroic," high-cost medicine; (2) the development of systems of instant communication; (3) dramatic increases in the speed of travel and transport; (4) the spread of robotology and gadgeteering that replace human labor; and (5) vast increases in the destructive force of explosives, both "conventional" and nuclear. What ties these disparate innovations together is not only the quantum leap of control over natural forces, but also their capacity to disrupt existing institutions at every level of human existence. They do so by giving the individual unprecedented leverage over society (and the state) and individual societies unprecedented leverage over the larger community of nations. These technologies may lead to a decline in the integrity of national states. They will lose control over their resources just as demand for services and benefits rises, not least on the part of those who have been rendered unemployed by fugitive capital and robotology. As a corollary, the international system, which has shown surprising stability and continuity over the past two hundred years, will likely experience greater and more random mobility and circulation of societies. Capital will migrate and hitherto weak and poor entities may be able to take advantage of the decoupling of military power from elaborate economic infrastructures by acquiring and using unconventional weaponry in their quest for international mobility.

Unlike the postindustrial paradigm, that of postmaterialism draws heavily on the idealist legacies of empirical sociology. As a social phenomenon, this mode of thinking is closely associated with the left, a remarkable development in view of past precedent. The association began with Gramsci's expropriation of Pareto and culminated in the democratization of Nietzsche by the radical wing of the American intellectual community. In the process the classical idealist notion of the "self-discovery of man" became collectivized: *cogito* became discourse, and discourse the creator of culture. If idealist social science and historiog-

[73] See Robert B. Reich, *The Work of Nations* (New York: Knopf, 1991); Paul Kennedy, *Preparing Ourselves for the Twenty-first Century* (New York: Random House, 1993). The summary below draws upon these works but goes beyond their authors' conclusions.

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raphy previously focused on the culture-creating role of the hero (Carlyle), the superman (Neitzsche), the elite (Pareto), or the charismatic leader (Weber), culture now became the product of every man. Intellectually, the product is a cultural paradigm, breeding "cultural theory."

The looming figure of this revival, Jürgen Habermas, goes one step beyond cultural theory with a grand synthesis of idealism and materialism—his self-described attempt to save the "enlightenment project" of empirical sociology.[74] He is in that tradition not merely because of the scope but also because of the starting point of his enterprise: the dynamics of history are driven by man-to-man (rather than man-to-nature) relations and, when all is said and one, his subjects are motivated by ideal interests. Much like Weber and Parsons, but more succinctly than either of them, Habermas strikes a balance between material and ideal interests by assigning them to stages of historical development. In the premodern age, life, culture, and politics were dominated by interest in the fate of one's immortal soul, reflected in the political institutionalization of divine right. In the modern period of capitalist industrialism dominant interests are material. Institutions reflect the tenets of secular utilitarianism, and culture is reflective of economic interests, or, in Habermas's evocative language, the "life world" is being colonized by the material world.[75] The postmodern world, then, is marked by a return and a dominance of ideal interests, expressed by a new culture of aestheticism and by a new politics of empathy. Like the schemes of Weber and Parsons, this one is developmental and hence highly ethnocentric, made even more so by the prediction that postmaterial consciousness will be diffused from the West to the other, less developed parts of the world.

While rooted in dialectics, the true inspiration for this scheme has been the aestheticism and environmentalism of the new left in the West. But herein also lies the vulnerability of the paradigm, as indeed of paradigmatic thinking in more general terms. Whether or not this is admitted, the method of construction is extrapolation. But do we extrapolate from a critical phenomenon or from a marginal one? Needless to say, the rise of green movements coexists with rampant economy mindedness and consumerism. If in doubt, we need only remind ourselves of the winning slogan of the 1992 American elections—"the economy, stupid"—or survey the global landscape for the manifesta-

[74] Habermas, *The Theory of Communicative Action*, vol. 1, trans. Thomas McCarthy (Boston: Beacon, 1982), xvii–xix.

[75] Ibid., 1:71–72, 340–43; and 2:153–98. Also Habermas, "Toward a Reconstruction of Historical Materialism," in Steven Seidman, ed., *Habermas on Society and Politics* (Boston: Beacon, 1989), 114–42.

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tions of that ubiquitous international demonstration effect. As Bauman, sympathetic to postmaterialism, observes, in 1989 "well-fed and clad, educated . . . young East German professionals stampeding to the West did not pretend to be running away from a disliked political philosophy; when pressed they admitted that what they were after . . . (was) a wider selection of goods in the shops and a wider selection of holidays."[76] Bauman also writes about "the enticing and alluring spectacle of lavish consumption enjoyed under capitalist auspices" and "the narcissistic culture of self-enhancement, self-enjoyment and instant gratification and life defined in terms of consumer styles,"[77] which not only attracted refugees from the socialist East, but continues to shape lifestyles and aspirations all over the world. The fundamentalists' relentless struggle against this "cultural imperialism" is perhaps the best proof of the continued attractions of this material worldview.

The problem of postmaterialism then raises a more general question about the paradigm concept and its potential pitfalls. While Kuhn has been more preoccupied with situations in which new phenomena destroy old harmonies, today we encounter the dilemma of having two paradigms, each of which can accommodate a new reality while giving them two separate, indeed, contrary, meanings. Thus if we look at the ecology movement through the prism of neoidealism, we will see a group of dedicated "idealists," people who renounced the crass materialism of modern life for higher aesthetic and altruistic purposes. But should we, as do their detractors, look at them through the prism of conventional sociology, we may see their actions as driven by power, as the ultimate revenge of *homo ludens* over *homo faber*. Or, still more harshly, we may see the phenomenon as a desperate cry for status in new bureaucracies on the part of children of the educated middle classes, left behind by ever more competitive markets and possessing no special entitlements to the social goods now distributed to the underprivileged by democratic states. The task here is to adjudicate between the validity of two paradigms, rather than building a new one out of the shambles of the old.

Conclusions

The Utility of Paradigms

This article was introduced by references to the paradigm concept as it emerges from the works of Thomas Kuhn and Ludwik Fleck. Whether by sheer coincidence or by conscious borrowing, these two works dis-

[76] Bauman (fn. 60), 171.
[77] Ibid.

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play similarities in conceptualization and argument. These include the notion of an exemplar, that of a scientific community (*Denkkollektiv*), and a detailed description of paradigm crises. Fleck's *Denkkollektiv*, like Kuhn's "scientific community," starts out with a consensus undergirded by absolute certainty, within which "contradiction to the system appears to be unthinkable," then moves to "laborious efforts" to explain "exceptions in terms that do not contradict the [familiar] system,"[78] eventually to resolve the contradiction by *Sprengung*[79]—a term that suggests both explosion and leap—thus clearly anticipating Kuhn's concept of the "breakthrough."

But the differences between these two texts are also substantial. On the one hand, Fleck is clearly and unambiguously influenced by German phenomenology and subjectivism, conceiving of facts as artifacts, as products of a collective state of mind or as reflections of consciousness. He tells us that consciousness evolves through the "conversation of two persons,"[80] with language embodying "compelling philosophy,"[81] and the facts change only when this philosophy changes. Indeed, with these epistemological postulates, Fleck may be celebrated as a forerunner of the contemporary postmodern theory of knowledge.

In contrast, Kuhn's scheme remains rooted in an aprioristic point of view, with facts firmly in command. To be sure, his paradigm is an intellectual construct that is not free from sociocultural influences, such as habituation, deference to established authority, fear of taboo, psychological mechanisms of ego defense, or even the crasser impulses of self-interest. But while these may inhibit the transformation of knowledge, in the end empiricism prevails. Whether he writes of the great leap made by Newton or Lavoisier,[82] the discovery of the Leyden jar in electronics,[83] or the role of the telescope in the ultimate victory of Copernican astronomy,[84] new techniques of observation show that "something was awry" and that there were phenomena that "could no longer be fitted to [preexistent] perceptual categories."[85] This is important to underline, for more recently there has been a trend to view Kuhn not only as the person who introduced us to a randomized, nonteleological view of change but as a precursor of the postmodernist critique of empiricism.

[78] Fleck (fn. 2, 1935), 35; (fn. 2, 1979), 27.
[79] Ibid., 9.
[80] Fleck (fn. 2, 1979), 44.
[81] Ibid., 42.
[82] Kuhn (fn. 4, 1962) 2.
[83] Ibid., 61.
[84] Ibid., 153.
[85] Ibid., 115.

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The slice of intellectual history discussed in this essay seems to confirm Kuhn's view of paradigms and paradigm change. While it is easy to find in this history of political sociology (and comparative politics) instances when habit or taboo—fear of violating a beloved object directly or indirectly—have impeded the rise of new knowledge, in the end it appears, as Peter Katzenstein recently observed, that "world events were driving scholarship" in comparative politics.[86] Thus while the classical paradigm of developmental and social systemic changes long held their own because they were intellectually and emotionally supported by the American experience of technological superiority cum democracy, a revolution in the gathering of statistics worldwide eventually focused attention on the persistence of international economic inequalities and attracted converts to the new paradigm of globality. Likewise, while economic explanations were favored both because of their neatness and on emotional grounds, the observation of concrete instances of the impact of external power has slowly begun to bring back the ideas of militarism and hegemonic power into analytic focus, if only via scholars who, as relative outsiders, have been immune to disciplinary pressures.

As we have seen, the rise of paradigms is not without peril. Just as in the case of resistance to change, so in the case of innovation, cultural and psychological factors may play a role. But most frequently scholarship may be led, or misled, by one of the most common impulses of social science, to move too quickly from the particular to the general and from the general to the universal. Thus one may recall how the industrial revolution of a single country, England, produced one of the most powerful economic paradigms of history, how the sudden spurt of development in the NICs led to the rise of the "new institutionalism," or how the much publicized presence of pressure groups with altruistic slogans inspired the contemporary renaissance of idealism in postmodern sociology. The Kuhnian conclusion to be drawn from these examples is that there may be noxious power in numbers. The challenge to the individual is to be able to negate what is obvious to the larger scholarly community. This requires intellectual courage, which, like civil courage, is not a commodity in overabundance, though it should be part of the baggage of both practicing and aspiring social scientists.

To respond, then, to Hirschman's dilemma—a paradigm is both a necessity and a hindrance. On the one hand, it is hard to envisage

[86] Katzenstein, contribution to "The Role of Theory in Comparative Politics: A Symposium" (fn. 62), 14.

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meaningful theorizing without a rudimentary exemplar and some assumptions concerning human motivation and behavior. On the other hand, once a theory is formulated on the basis of past experience, the scholar must anticipate potential spatial and temporal limitations, which can beset even the best of analogies. To repeat what has been said before: theories consist not only of variables but of constants, and the theorist must anticipate that over time even the firmest of these constants may turn into variables. What Kuhn tells us is that there is no way out of this dilemma. The story of theorizing is one of both ecstasy and agony, the inescapable companions of all creative enterprise.

A Note on Probabilities and Possibilities

While paradigms raise questions, theory is the instrument for conveying answers that arise out of empirical research. In the social sciences such research involves observing frequencies—either by statistical methods or, where this is not possible, by searching for analogy over time and place. The constructs that result are imperfect, partly for the reason just noted and partly because, as is generally understood, theories of social science do not emerge from laboratories. Instead they emerge from configurations of events, and each one of these configurations leaves open the possibility that the real causal agent has been some undetected circumstance, an "impurity," in laboratory terms. As a result, these theories always point to probabilities and in the strict sense should be considered less as instruments of prediction than as initiators of informed debate about yet uncompleted sequences of events.

Albert Hirschman, who provided the point of departure for this study, is clearly dissatisfied with this kind of theory and juxtaposes the respective utilities of probability and possibility. This juxtaposition deserves serious examination and, above all, requires definitions of the two terms at hand. The dictionary definition of probable is that which is "likely to occur or be; that can be reasonably, but not certainly expected." In contrast, that which is possible is described as something "that can be done, known, acquired or selected *depending on circumstance*.[87] The major distinction between the two is circumstance, and this takes us beyond analogy and statistical frequency. Its calculation is a different kind of exercise, demanding imagination rather than strict intellectual discipline.

The relationship between the two concepts is complex as well as paradoxical. On the one hand, as Clausewitz tells us in his introduction

[87] *Webster New World* (Cleveland, Ohio: World, 1975), 1112, 1132, emphasis added.

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to the study of war, one will not win a war by merely following the rules of engagement reflecting past experience and, indeed, probability. To borrow his more powerful imagery: "Pity the poor warrior who is contented to crawl around in the beggardom of rules."[88] The same applies to players of chess, or cards, or any other game that takes place in the fog of uncertainty. Indeed, calculations based on past behavior alone may even predict a net loss. The race sheets sold at the tracks note that strict adherence to their tables of probability is likely to result in just such an outcome. The paradox is that people still buy these sheets and use them, by adjusting them to circumstance as they perceive it. The same goes for bridge players who, while profiting immensely from established rules of prudence based on probability, will become world class only if blessed with the intuition that tells them when to break away from the statistically established rule. Clausewitz tells us that in the fog of war—or under any general condition of uncertainty—the commander has to rely on "a certain power of discrimination" and "a natural mental gift of a special kind."[89] Yet at the same time, Clausewitz also cautions the commander to avoid the extreme, which should not be sought for its own sake, and to use judgment to determine the limits of his efforts, which "can be done only on the data furnished by the facts of the real world and by the *laws of probability*."[90]

There is some question whether the construction of such rules and leadership itself can be combined within the same individual. Clausewitz is skeptical and, in any event, warns the staff officer to leave his theories at home when entering the field. There is no doubt, however, that we deal here with two distinct social roles. Certainly, one can be a good analyst without having the special gift of dealing with uncertainty. Indeed, brilliant economists have often proved to be less than successful entrepreneurs, and political scientists do not necessarily make good political professionals.

If there are two distinct roles, then, we may also find that there exist two separate role cultures, each imposing its own set of moral obligations. The commander's obligation is to weigh the experience of the past before also weighing the costs of failure against the benefits of victory. While this imposes the obligation to respect the laws of probability in order to diminish the chance of failure, it may also impose the obligation to act against desperate odds—if the alternative is surrender or certain death. General Custer comes to mind here.

[88] Clausewitz (fn. 45, 1943), 43.
[89] Ibid., 28, 31.
[90] Ibid., 157–58, emphasis on original.

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Cast in a less heroic mold, these rules of engagement apply to political leadership. As in the past, the likelihood of development and self-sustaining democratic government is not exceedingly high in many parts of the world today, including the countries of the postcommunist world. Nonetheless, leaders must take charge and do their best, because the alternatives are protracted social disorganization and decay. None of this, however, obligates the academic to conceal the dismal lessons of history, any more than it obligates the statistician to conceal the dismal odds of gambling in order to engender greater optimism. Scholars should by all means point out the limits of analogy but should be wary of acting as cheerleaders and protagonists of the unusual. If their ambition is to help whomever they define as the wretched of the earth, they must be ready to abandon their ivory towers and take to the field, with all the attendant risks to life and limb.

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PROBLEMATIC LUCIDITY
Stephen Krasner's "State Power and the Structure of International Trade"

By ROBERT O. KEOHANE*

INTERNATIONAL political economy is a subject without clear boundaries. It is as old as trade and theft between societies—hence older than written history. It is spatially extensive: today, no spot on the globe is untouched by distant markets or their manipulation by powerful states, and if intergalactic trade were ever to become a reality, international political economists would surely study it. The actual agents number in the billions: they are firms and individuals, from parochial peasants to jet-setting tycoons and office-bound state bureaucrats. Unlike stylized versions of "international" (that is, interstate) politics, political economy cannot be encompassed solely by the interactions of states.

This absence of boundaries implies an abundance of confusion. Unable to delimit our subject conveniently, everything may appear relevant to us, from factor endowments to electoral systems, exploitation of labor to exploitation of the natural environment. Realizing that seeking to understand everything will lead to knowledge of nothing, we desperately seek threads to guide us through the labyrinth, to show us what is important and how different forces and processes are related. Clarity and simplification become essential.

We know, however, that predictability is elusive. Too many factors interact in complex ways to produce the results we see. Random shocks disrupt the system. Patterns that first appear for nonsystematic reasons become locked in, in path-dependent ways. However boldly we press our favorite explanations or approaches, we recognize that they are all partial, at best—no more than approximations of how the international political economy works. The clearer the argument, the more obviously problematic it may be.

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As students of international political economy, we are attracted to lucidity. We want a coherent conception of our subject, even though we recognize that any such conception will be partial and flawed. Particularly lucid arguments can therefore stimulate great scholarly interest and productive responses, even if they are conceptually and empirically inadequate. Although we are never likely to be able to predict or thoroughly explain specific strategic interactions among states, firms, and nongovernmental organizations, we can aspire to conditional generalizations that narrow the range of our uncertainty by accounting for general patterns of behavior.

Stephen D. Krasner's 1976 article in *World Politics*, "State Power and the Structure of International Trade," is notable for its distinctive combination of lucid and problematic propositions.[1] Krasner forcefully argues that openness in the world economy is most likely to occur "during periods when a hegemonic state is in its ascendancy" (p. 323). As long as the state's technological lead is increasing, its leadership will perceive economic advantages to openness, since openness will expand markets for the products of its technologically sophisticated industries. The hegemon will also gain politically, since the "opportunity costs of closure" will be low, relative to those facing smaller and poorer states. Conversely, when several large, unequally developed states coexist, Krasner predicts that the more backward states will find openness economically and politically costly and will therefore resist it. Greater trade closure will therefore result.

"State Power and the Structure of International Trade" crystallized issues and set the terms for more than a decade of work in the field of international political economy (IPE). Although several of Krasner's themes can be found in the earlier writings of others, no one juxtaposed economics and politics more succinctly or effectively than he. And no one combined lucidity with problematic argument so well. By raising key issues in a pungently provocative way and by manifestly resolving none of them, Krasner defined the agenda for years of scholarship.

I. Major Themes and Causal Mechanisms

"State Power and the Structure of International Trade" operates simultaneously on three levels. Its first sentences announce a *program* for the revival of the state in studies of international relations: "In recent years, students of international relations have multinationalized, transnation-

[1] Krasner, "State Power and the Structure of International Trade," *World Politics* 28 (April 1976).

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alized, bureaucratized and transgovernmentalized the state until it has virtually ceased to exist as an analytic construct. . . . This perspective is at best profoundly misleading" (p. 317).

Krasner's rhetoric is dazzling: the core subject matter of political science is threatened and must be recovered. "State Power" sounds the rallying cry for the defenders of the state. People who look to the state for redress of market unfairness, scholars whose human capital is invested in understanding how states work, and Hobbesian skeptics suspicious of visions of efficiency and harmony can all rally around the statist standard. Teaching and writing on this theme in the mid-1970s, after the defeat of the United States in Vietnam, Krasner helped to launch a counterwave of renewed interest in the state against the trends emphasizing economic interdependence, transnational relations, "ungovernability," and the state's alleged economic irrelevance. Leading economists had been more impressed with the advantages of multinational firms over states. Not long before Krasner wrote, Charles Kindleberger had proclaimed that "the nation-state is just about through as an economic unit."[2] Even political scientists sympathetic to the transnationalist research agenda recoiled from such rhetoric. It was Krasner's counterrhetoric that energized a statist reaction—and it mattered little that his article nowhere defines "the state"; contests over definitions also became part of the subsequent scholarly debate.

Some of the appeal of "State Power" derives from its clear specification of a *puzzle*. Puzzles are central to social science, and Krasner's puzzle is an important one. Why, he asks, has the world economy vacillated between openness and closure? To clarify this issue, Krasner carefully defines the continuum between openness and closure. While acknowledging the significance of movements of capital, labor, and technology, he focuses on trade. He argues that openness or closure in the structure of international trade can be operationalized by examining tariff levels, trade as a proportion of national product, and the regionalization or globalization of trade. Krasner does not merely present an important puzzle; he provides a way to measure the dependent variable that he has identified. Thus he lays out the basis for a focused research program.

"State Power" also advances a strong *proposition*, as stated above, that hegemonic ascendancy tends to create openness. Indeed, Krasner proposed a set of explanatory variables—position in the world political economy, defined in terms of size, level of development, and changes in

[2] Kindleberger, *American Business Abroad: Six Lectures on Direct Investment* (New Haven: Yale University Press, 1969), 207.

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relative economic predominance—to account for the dependent variable of openness. Hence from the outset he emphasized theoretically meaningful questions. For political scientists, Krasner's choice of a master explanatory variable was especially appealing, since in his formulation, power ("our" subject) determines economic patterns, rather than vice versa.

However bold the hegemonic-power formulation, however, there is a serious problem: within twenty-five pages Krasner himself seems to falsify his own proposition. Indeed, he finds that hegemony correctly predicts openness in only three of the six periods that he considers (in 1820–79, 1880–1900, and 1945–60, but not in 1900–1913, 1919–39, or 1960–75). He admits that "the whole pattern is out of phase" and in the last three pages of his article proposes an amendment, arguing that "some catalytic external event seems necessary to move states to dramatic policy initiatives in line with state interests" (p. 341). Such events, however, seem to be known only post hoc. World War I was a dramatic event but did not prompt the United States to invest in leadership of the world economy, whereas the Great Depression and World War II combined to do so. So far, neither the gradual decline in its dominant postwar position in the world political economy nor the end of the cold war has propelled the United States back to protectionism. In the end, therefore, what Krasner offers is less a rigorous explanation than a brilliant interpretive guide to the past, with post hoc qualifications.

How is it that an article whose central argument seemed to implode in under twenty-five pages has been so influential in the field?

My thesis is that Krasner's article was influential partly *because* his argument, while lucid and insightful, contained gaps in its causal arguments, as well as empirical anomalies. The anomalies—Britain's support of openness after 1900, the failure of the United States to exercise leadership after 1919, and arguably (within Krasner's framework) U.S. support of openness after 1960—practically leap off the page. Once Krasner had identified openness as a dependent variable, the anomalies demanded explanation. But perhaps more important, Krasner sketched an argument about why hegemony *should* engender openness. Aspects of this argument were both problematic and immensely fruitful.

II. Krasner's Economics

Krasner begins his causal argument by referring to standard economic doctrines of static efficiency, which hold that openness raises aggregate

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national income. Naïvely, one might therefore expect all states to pursue policies of free trade, but of course they do no such thing. One could seek to account for protectionist policies by assuming that the relevant policymakers were dumb or ignorant; but as Krasner observes, "Stupidity is not a very interesting analytic category." Instead, he proposes to explain variation in policies by assuming "that states seek a broad range of goals," of which he emphasizes four: aggregate national income, economic growth, political power, and social stability (p. 319).

As Krasner sees it, a state selects policies on the basis of its position in the world political economy, in light of these four goals. In particular, states' policy preferences reflect their size and level of development, as compared with those of other states. To understand variation in policies, therefore, the investigator must assess the *causal mechanisms* that link size and level of development with states' preferences for openness or closure. While Krasner's portrayal of these mechanisms is sketchy and controversial, his stress on causal mechanisms helped to focus future scholarship.

Krasner accepts the standard economic argument that all states benefit, in static economic terms, from openness. Small states benefit most, both in static and dynamic terms, from international trade. Therefore, a system of small, highly developed states will tend to be an open one. Under these conditions, economic interests lead to a liberal outcome. As we will see, this is an important conclusion, although it is not the point for which Krasner's article is famous.

For other states, the relationship between international economic structure and economic growth is "elusive." For large states, especially those without a strong technological edge, Krasner argues that the evidence is not yet in. On economic grounds alone, he reasons, large, highly developed states should favor openness at least as long as they can maintain their technological edge; large, less developed states or wealthy states that are losing their technological advantages are expected to be more inclined toward closure.

This conclusion depends on two key, and contestable, propositions. The first of these is that hegemonic powers should increasingly favor closure after they begin to lose their technological edge. I will call this Krasner's "*decline-closure* proposition." The second contestable economic proposition can be dubbed the "*backwardness-closure* linkage," according to which large, economically backward states will pursue policies of closure, for two reasons. First, they gain less from openness (being large and with factors of production that are difficult to move

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from one use to another), and second, openness makes them politically vulnerable to pressure from more powerful states.

The decline-closure proposition is crucial to Krasner's distinction between hegemony in a static sense (in which one state is larger and more advanced than its trading partners) and "hegemonic ascendancy." Ascendancy requires not only that a single state be larger and relatively more advanced than its trading partners but also that its relative size and technological lead be increasing (p. 322). Thus, according to Krasner, if the most powerful and technologically advanced state finds its lead diminishing, as Great Britain did in the thirty years before World War I, openness will work to its disadvantage by diverting resources from the domestic economy and by "providing potential competitors with the knowledge needed to develop their own industries" (p. 320). Accordingly, a hegemon's interest should switch from openness to closure after its lead over rivals has peaked.

This argument is not fully articulated. Perhaps it rests on the assumption that closure of one's borders to economic exchange, especially trade, would enable a leading country to preserve its advantage in knowledge. (That is, a state that eschews export opportunities and the benefits from corresponding imports will be better able to keep its trade secrets.) However, the assumption that closure to trade would prevent the diffusion of knowledge and hence preserve existing technological advantage is a questionable one. It seems more likely that knowledge would spread despite attempts at closure and that leading countries choosing closure would become less innovative and consequently lose their advantages more quickly than countries that maintained openness to the world economy.

Even more compelling are the political objections to the view that declining hegemons will develop preferences for closure. Governments that are accountable to domestic economic interests are unlikely to seek to thwart the efforts of their leading manufacturing industries to promote exports. The history of export controls designed to promote national defense suggests that in democracies it is difficult to impose such constraints even in situations defined as national emergencies. Indeed, pressures for closure are more likely to focus on imports than on exports and to emanate from import-competing rather than export-oriented sectors and social groups. Krasner's hypothetical state seeks the public good of national advantage, but he does not explore the conditions under which such a state's internal institutions would enable it to overcome fragmented interests and problems of collective action.

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Yet another line of support for the decline-closure argument could be more substantial, although it was not available to Krasner when he wrote: this is the strategic trade argument.[3] When industries are characterized not only by economies of scale but also by learning effects, cumulative output affects costs. That is, a firm's production costs may be a function in part of how much it has already produced. Suppose that the United States controls a huge market, Thailand has a tiny market, and that other markets are relatively closed to Thai goods. If the United States closes its markets to Thai goods, Thai production costs will remain high, since its firms will be unable to take advantage of either economies of scale or learning resulting from large-scale production. If the U.S. opens its markets and Thai firms have underlying sources of competitive advantage, their costs will fall. U.S.-based firms and their workers may put pressure on the United States government to close U.S. markets to products that compete with their own; if the advantages of U.S. firms over foreign competitors are declining in general, such pressures may be expected to be more severe. Since the United States market is so much more important to Thailand than the Thai market is to the United States, the United States may be able to extract compensation from Thailand for keeping them open. Krasner did not articulate such a logic, but he may have had something of this sort implicitly in mind. At any rate, the invention of strategic trade theory a few years after the appearance of "State Power" provided potential theoretical underpinnings for one of his key arguments.[4]

Krasner's other economic proposition links economic backwardness with policies of closure. This backwardness-closure linkage may have seemed fairly obvious in 1976, when dependency theory was at its apex and China was pursuing policies of autarchy, but it no longer seems sound. China is an economic dynamo, however ecologically destructive its rapid growth may be. Its growth depends on its relationships with the world economy and in particular its huge trade surplus with the United States, whose open market serves as an engine of Chinese growth. China devised a strategy that has enabled its elites to maintain

[3] I am indebted to Ronald Rogowski for pointing out to me that strategic trade theory could support Krasner's closure–relative advantage proposition. For an early work on strategic trade policy, see Paul R. Krugman, ed., *Strategic Trade Policy and the New International Economics* (Cambridge: MIT Press, 1986). David Lake also discusses this issue, in "Leadership, Hegemony and the International Economy: Naked Emperor or Tattered Monarch with Potential? *International Studies Quarterly* 37 (December 1993).

[4] Skepticism and controversy abound over other questions: whether democratic governments actually act on the basis of strategic trade considerations (rather than principally reacting to societal pressures) and whether such governments would be capable of intelligently crafting policies along strategic trade lines.

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their grip on power—avoiding the societal disruption that would be likely to result from genuine openness to the world economy but at the same time garnering substantial benefits from its global connections.

The Chinese example illustrates the limitations of a dichotomy between "openness" and "closure," understood as unilateral strategies. For China, *selective* openness and manipulation of the openness of others may be a superior strategy to a unilateral policy of either openness or closure. For leading powers such as the United States, conditional acceptance into the world trading regime of countries such as China—demanding their adherence to established rules as a price of admission—may be preferable to either unconditional inclusion or attempts at exclusion.[5]

III. Krasner's Political Arguments

Krasner's political and social propositions, rather than his economic arguments, were the principal source of his article's influence. Politically, Krasner argues, the *opportunity costs of closure* are crucial. Since these costs are relatively low for large, developed states (which have low ratios of foreign trade to production), these states can credibly threaten, in an open economy, to block international trade or investment to "secure economic or noneconomic objectives" (p. 320). Large states not only benefit economically from openness; they also gain politically. "A state that is relatively large and more developed will find its political power enhanced by an open system because its opportunity costs of closure are less" (p. 320). Social stability is also important. *Societal adjustment* to change will be more difficult in small states than in large states, because of their greater exposure to the world economy, and "social instability is thereby increased" (p. 319). Hence, although small states gain most in purely economic terms from openness, in Krasner's view the political and social incentives for openness are greater for large, relatively more developed states.

Krasner's propositions about the opportunity costs of closure and social instability were so intriguing yet so clearly incomplete that they stimulated efforts to identify and specify the operation of causal mechanisms, both those linking hegemony with openness and those that could tend to weaken or confuse such a linkage. Both sets of causal mechanisms provide windows on to subsequent work in the field. Since these

[5] I am indebted to Stephen Krasner for his observations on an earlier version of this essay, which stimulated this point.

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causal mechanisms played such a central role in Krasner's analysis and in its impact on the field, my review of the significance of his work focuses on these causal mechanisms and their treatment in previous and subsequent literature. This section focuses on the opportunity costs of closure (a concept that at the time Krasner wrote was becoming a core proposition of international political economy). The next section discusses the literature related to Krasner's point about societal adjustment.

Krasner's title, "State Power and the Structure of International Trade," was meant to evoke Albert Hirschman's book *National Power and the Structure of Foreign Trade.* Hirschman defined the "influence effect of foreign trade" as resulting from dependence of one nation on another and argued that "the classical concept, gain from trade, and the power concept, dependence on trade, are seen merely as two aspects of the same phenomenon."[6] In 1970 Kenneth N. Waltz asserted that "in international relations interdependence is always a marginal affair," a "myth." Waltz provided no empirical evidence for his claim, which rapidly seemed outdated as trade and investment soared as proportions of national product. Waltz's more important contribution was to draw a distinction between two types of interdependence:

> States are mutually dependent if they rely on each other for goods and services that cannot easily be produced at home. That kind of interdependence is difficult (costly) to break. The other kind of interdependence—sensitivity of response to variations in factor prices—may be economically more interesting; it is also politically less important.[7]

Drawing on Hirschman and Waltz, Krasner focused on "the relative cost of closure" and argued that "the higher the relative cost of closure, the weaker the political position of the state" (p. 320).[8]

The research program revolving around the opportunity costs of closure was not particularly novel, as David Baldwin pointed out in these pages in 1979.[9] The issues that arise are standard bargaining problems, well known in political science. Yet without making this connection between "state power and the structure of international trade," we

[6] Hirschman, *National Power and the Structure of Foreign Trade* (1945; reprint, Berkeley: University of California Press, 1980), 18.

[7] Waltz, "The Myth of National Interdependence," in Charles P. Kindleberger, ed., *The International Corporation* (Cambridge: MIT Press, 1970), 210.

[8] Joseph S. Nye and I expressed the same thought a year later in *Power and Interdependence*: "A useful beginning in the political analysis of international interdependence can be made by thinking of asymmetrical dependencies as sources of power among actors." Robert O. Keohane and Joseph S. Nye, Jr., *Power and Interdependence: World Politics in Transition* (Boston: Little, Brown, 1977), 18.

[9] Baldwin, "Power Analysis and World Politics: New Trends versus Old Tendencies," *World Politics* 31 (January 1979).

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would not have a *political economy* of international relations. Krasner's article is important for having helped to clarify and crystallize this connection.

More important than Krasner's discussion of the opportunity costs of closure for individual states is his attempt explicitly to take into account the concern of states for security and to link arguments about security with his opportunity costs argument. Krasner's discussion of economics and security builds on Robert Gilpin's contention that the expansion of international economic activity was dependent in the nineteenth century on the *Pax Britannica* and after 1945 on the *Pax Americana*. In Gilpin's view, echoed by Krasner, "the pattern of international economic relations is dependent upon the structure of the international political system."[10] States seek security as well as wealth. Because power is essential for security, and wealth is a means to power, powerful states will seek to structure international economic relations in a way that generates wealth and power for themselves. Krasner sought to specify and generalize Gilpin's argument, proceeding in two steps: (1) states in hegemonic ascendancy will favor openness for political as well as economic reasons; and (2) since such states are hegemonic, they will by definition have the capacity to liberalize the world trading system as a whole. "The potentially dominant state has symbolic, economic, and military capabilities that can be used to entice or compel others to accept an open trading structure" (p. 322). Hence, the world economy will be more open when dominated by an ascendant hegemon than when a few large states coexist or when the hegemon is declining. In general, the world economy will be most open at two extremes: with many small states, each with a strong interest in economic openness, or with one ascendant hegemon. By generalizing Gilpin's argument and specifying a clearly defined dependent variable, Krasner put it into a form conducive to social scientific analysis.

Krasner's argument about hegemony, while similar to Gilpin's, differs sharply from that of Charles P. Kindleberger, who concluded from his study of the Great Depression that one state must provide the public good of stability: "For the world economy to be stabilized there has to be a stabilizer—one stabilizer." Kindleberger's argument combined functional logic with the theory of public goods. Krasner was concerned with openness, not systemic stability in a crisis, and he relied on

[10] Gilpin, "The Politics of Transnational Economic Relations," in Robert O. Keohane and Joseph S. Nye, eds., *Transnational Relations and World Politics* (Cambridge: Harvard University Press, 1972), 54–55. *Transnational Relations and World Politics* first appeared in a special issue of *International Organization* 25 (Summer 1971).

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neither functional logic nor public goods theory.[11] In a recent article, David Lake even gives the two formulations different names, referring to Kindleberger's version as "leadership theory" and Krasner's as a neorealist version of "hegemony theory."[12]

Krasner articulated most clearly what I later dubbed the "theory of hegemonic stability"—the theory that strong international economic regimes depend on hegemonic power. My label did not really do justice to Krasner's argument, since his dependent variable was openness rather than the strength or stability of international regimes (in which I was more interested), but the label stuck even after its source was largely forgotten.[13] The so-called theory of hegemonic stability generated lively scholarly controversy, focusing on three major issues: (1) the logical basis of the arguments made by Gilpin, Kindleberger, and Krasner; (2) whether without a hegemon a small number of countries could maintain openness through strategic interaction, and if so, under what conditions; and (3) what the empirical record showed about the relationship between structure and trade openness.

John Conybeare put forward a logical objection to the hegemonic stability argument by pointing out that the optimal policy for a hegemon should be "monopolistic predation," as in the form of an optimal tariff. However, Conybeare conceded that hegemons "have in some cases pushed for open systems." He speculated that "this (possibly) second best strategy may be nothing more than a reflection of the hegemon's perception of the credible deterrence capacities of small powers."[14] Five years later, Joanne Gowa used Conybeare's own analogy to refute his argument. Like monopolists following practices of "limit pricing," hegemonic states have incentives to limit their short-term exploitation of others in order to discourage challengers. Hence Gowa provided an additional sophisticated theoretical rationale for the association of hegemony with openness that Krasner expected to find and that is observed in the second half of the nineteenth and twentieth cen-

[11] See Kindleberger, *The World in Depression, 1929–1939* (Berkeley: University of California Press, 1973 and subsequent editions). The quotation is on page 304 of the revised and expanded edition of 1986. Krasner accepts this distinction between his work and Kindleberger's. See Michael C. Webb and Stephen D. Krasner, "Hegemonic Stability Theory: An Empirical Assessment," *Review of International Studies* 15 (April 1989), 184.

[12] Lake (fn. 3).

[13] Robert O. Keohane, "The Theory of Hegemonic Stability and Changes in International Economic Regimes, 1967–1977," in Ole Holsti et al., eds., *Change in the International System* (Boulder, Colo.: Westview Press, 1980), 136. Vinod Aggarwal most forcefully pointed out that international regimes could be illiberal. See Aggarwal, *Liberal Protectionism: The International Politics of Organized Textile Trade* (Berkeley: University of California Press, 1985).

[14] Conybeare, "Public Goods, Prisoners' Dilemmas and the International Political Economy," *International Studies Quarterly* 28 (March 1984), 12.

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turies under British and American leadership, respectively.[15] As Gowa argued, the implication of Conybeare's optimal tariff argument was not so much that hegemony should produce closure (which would only have compounded empirical anomalies) but that strategic interactions must be explicitly modeled to account for observed policies.

Such strategic arguments were made informally in 1984 by David Lake and myself. Lake used Krasner's structural arguments to argue *against* the thesis that hegemony would be necessary for liberal trade. For Lake, countries with high relative productivity and medium relative size should be "supporters" of liberal trade. He took the view that the trend in the international economic structure is toward "multilateral supportership."[16] In a sense, Lake used Krasner's structural approach to refute Kindleberger's argument about the necessity of a hegemon for a liberal economy. In *After Hegemony*, I argued that under favorable historical and institutional conditions a small number of countries, each concerned about linkages among issues and about their reputations, could maintain cooperation among themselves on the basis of shared interests.[17] Neither Lake nor I challenged the proposition that hegemony and liberal trade were empirically associated, although we both questioned the *necessity* of hegemony for openness. Our arguments were complemented and reinforced a year later by Duncan Snidal, who used game theory to make the case more rigorously that small groups can have incentives to cooperate and that under some conditions, "hegemonic decline strengthens the possibility of collective action by forcing states to cooperate if they wish to achieve reasonable outcomes."[18]

The increasing emphasis on strategic interaction in this literature did more damage to Kindleberger's leadership version of hegemonic stability theory, with its emphasis on the need for the leader to provide public goods, than to Krasner's structural theory. Indeed, this strategic orientation was actually consistent with Krasner's way of thinking about world politics, although in 1976 he did not explore strategic issues explicitly.[19] By coupling this strategic orientation with the tradi-

[15] Gowa, "Rational Hegemons, Excludable Goods, and Small Groups: An Epitaph for Hegemonic Stability Theory?" *World Politics* 41 (April 1989), esp. 312.

[16] Lake, "Beneath the Commerce of Nations: A Theory of International Economic Structures," *International Studies Quarterly* 28 (June 1984).

[17] Robert O. Keohane, *After Hegemony: Cooperation and Discord in the World Political Economy* (Princeton: Princeton University Press, 1984).

[18] Duncan Snidal, "The Limits of Hegemonic Stability Theory," *International Organization* 39 (Autumn 1985), 611.

[19] In discussing neoclassical trade theory, Krasner mentions a strategic-interaction argument of Harry Johnson: "that the imposition of successive optimal tariffs could lead both trading partners to a situation in which they were worse off than under competitive conditions" (p. 318). Later work by

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tional realist emphasis on security issues, Gowa has argued that since trade produces security externalities, states will trade disproportionately with their allies.[20] In this view, cooperation among states is usually severely inhibited by competition for relative gains. But allies will not compete as strenuously as will potential adversaries to improve their *relative* positions vis-à-vis one another. Krasner himself has employed this line of argument to speculate about the persistence of cooperation in the 1980s, despite the absence of hegemonic ascendance. The major trading states were allied with one another, hence "need not be so concerned with relative gains from international economic co-operation with their alliance partners."[21] "Saving" the theory in this way is, however, a double-edged sword: the longer economic openness continues after the end of the cold war, the more dubious the interpretation will appear, as compared with the liberal-institutionalist view that emphasizes the capacity of a small number of states, linked by institutions, to cooperate for mutual gain.

Empirical arguments about hegemonic stability theory began to rage early in the 1980s. Timothy McKeown argued that the predictive accuracy of hegemonic stability theory is poor for nineteenth-century tariff levels, and John Conybeare made a similar claim for the 1902–71 period. Whereas McKeown suggested that tariff levels might be better explained by business cycles, Conybeare saw them as increasingly related to levels of development.[22] Later, McKeown performed systematic tests of the theory with data for over a century (1880–1987) and found little support for hegemonic stability theory.[23] Krasner himself, in an article with Michael Webb, pointed out that between 1960 and 1987 the world economy became more open despite the demise of American hegemony, dated by Gilpin, Kindleberger, and Krasner at some point between 1960 and the mid-1970s. Krasner and Webb observed that although "initial proponents of the theory pointed out that there would be lags between changes in the distribution of power and

Krasner has explicitly used a strategic formulation. See especially "Global Communications and National Power: Life on the Pareto Frontier," *World Politics* 43 (April 1991). In that article he (unfairly, in my view) claims that "market failure analyses, which have dominated the literature on international regimes, pay little attention to power" (p. 342).

[20] Joanne Gowa, "Bipolarity, Multipolarity, and Free Trade," *American Political Science Review* 63 (December 1989); idem, *Allies, Adversaries, and International Trade* (Princeton: Princeton University Press, 1994).

[21] Webb and Krasner (fn. 11), 196.

[22] McKeown, "Hegemonic Stability Theory and Nineteenth Century Tariff Levels in Europe," *International Organization* 37 (Winter 1983); Conybeare, "Tariff Protection in Developed and Developing Countries: A Cross-Sectional and Longitudinal Analysis," *International Organization* 37 (Summer 1983),

[23] McKeown, "A Liberal Trade Order? The Long-Run Pattern of Imports to the Advanced Capitalist States," *International Studies Quarterly* 35 (June 1991).

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changes in international transactions, . . . no systematic analysis of lags has yet been offered."[24]

Recent empirical work, however, may have come to the rescue of Krasner's argument, albeit with significant qualifications. Edward Mansfield has distinguished between hegemony (the relative inequality between the two largest powers) and the concentration of power in the system, measuring the "aggregate inequality of capabilities among all powers in the system."[25] He finds that whether hegemony is associated with openness depends on the measure of hegemony one uses (in particular, whether British hegemony is seen as ending in 1873, in which case hegemony is associated with openness, or in 1914, in which case it is not). Most important, Mansfield demonstrates that a U-shaped relationship exists between concentration and trade: trade is greater at low and high concentrations of capabilities than at middling levels. He points out that his findings—coming eighteen years after Krasner's work—confirm the latter's position "that the system is likely to be open when relative inequality is both highest and lowest."[26] After Krasner's most famous proposition appeared to have been empirically refuted, Mansfield provided evidence indicating that concentration of power, if not hegemony, is associated with trade openness. He also provided evidence to show that its much-neglected partner proposition—that a system of many small states is likely to be open—is also correct. This story suggests, as does much of the philosophy of science, that a plausible theory should not be abandoned too quickly in the face of facts that seem to disconfirm it, at least not until investigators have reflected on whether careful analytical distinctions and more systematic evidence might support a modified version of it after all.

The research program stimulated by the theory of hegemonic stability is both conceptually and empirically rich. Since Krasner's brilliant speculations were published, we have learned a great deal about the effects of hegemony and concentration on both openness and the stability of international regimes. Fortunately, the editors of *World Politics* in 1976 were undaunted by objections to Krasner's evidence and published his article.[27] Our field needs to be open to such original, if tenuous, flights of scholarly imagination!

[24] Webb and Krasner (fn. 11), 195. On the dating of the demise of hegemony, see pages 185–86.

[25] Mansfield, *Power, Trade and War* (Princeton: Princeton University Press, 1994), 179, 73.

[26] Ibid., 181. I am especially indebted to Professor Marc Busch of Harvard for pointing out the correspondence between Mansfield's findings and Krasner's theory.

[27] I claim no prescience in this respect. I recollect that my first reaction to Krasner's paper was more critical of the unsatisfactory nature of his evidence than appreciative of the lucidity with which he defined his variables and stated his key proposition. Since recollections are usually biased in favor of the reputation of the teller of the tale, this one is probably, unfortunately, correct.

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IV. Societal Costs of Adjustment and Comparative Political Economy

Krasner himself recognized the failure of his argument to explain the nature of the international trading system during three periods: 1900–1913, 1919–39, and 1960–75. His attempt to reconcile these anomalous periods with his theory relies on domestic politics, in particular, on the assumption that institutional arrangements, once established, are difficult to change: "Once policies have been adopted, they are pursued until a new crisis demonstrates that they are no longer feasible. States become locked in by the impact of prior choices on their domestic political structures" (p. 341).

Krasner's reliance on variations in domestic institutions, even as a post hoc explanation for anomalies, constituted an admission, from someone identified with an international structuralist standpoint, that major patterns in the world political economy could not be explained without attention to comparative politics. Pursuit of this insight by students of comparative politics quickly led to some illuminating work. In a 1977 special issue of *International Organization* its editor, Peter J. Katzenstein, followed Gilpin and Krasner in portraying "the cycle of hegemonic ascendance and decline," but argued that differences in policy among advanced industrialized states could be explained only through comparative analysis, by "focusing on the governing coalitions which define policy objectives and the institutional organization which conditions policy instruments."[28] Krasner himself contributed a chapter on the United States and argued that the liberal vision of vastly increased interdependence was flawed. "Interests, alone, have not been enough to constitute an international order. It has always been necessary to have some political power that can provide collective goods and enforce rules and norms." Although the United States maintained sufficient potential power resources, the weakness of the American state—that is, the ability of private groups to block state initiatives—would prevent the United States government from effectively mobilizing those resources. The "ideal policy for the U.S." would be to move toward closure; but the weakness of the American state and Lockean liberalism means that "a policy of controlled closure is no more likely to be pursued than one of renewed liberalism. In the future, U.S. policy is likely to become less coherent and the world economy more unsta-

[28] Katzenstein, "Introduction," in Katzenstein, ed., *Between Power and Plenty: Foreign Economic Policies of Advanced Industrial States* (Madison: University of Wisconsin Press, 1978), 7, 18. This volume first appeared as an issue of *International Organization* 31 (Fall 1977).

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ble."[29] Krasner's prognosis turned out to be incorrect; but his acceptance of the need for the analysis of domestic institutions as well as international power structures foreshadowed future work in the field.

In 1978 Peter Gourevitch turned the problem around, showing how international relations could affect domestic politics as well as vice versa He proclaimed that "students of comparative politics treat domestic structure too much as an independent variable, underplaying the extent to which it and the international system are part of an interactive system."[30] From this insight has flowed a stream of work about the impact of the international political economy on domestic politics that has reinforced Gourevitch's point. It would be difficult now to sustain the assumption, common two decades ago, that the politics of various countries could be compared without seriously examining their relative positions in the world political economy. To account for patterns in the world economy, we need to understand comparative politics; conversely, good domestic political analysis requires comprehension of the international context.[31]

V. Accounting for the Impact of "State Power": Some Speculations

Neither the lucidity of Krasner's argument nor its problematic quality fully explains its impact on the field of IPE. Quite a few lucid articles appear on important topics, but few receive the attention lavished on "State Power and the Structure of International Trade." Although I have no general explanation for the observed variation, I can offer some speculations, on the basis of personal experience, about why Krasner's article achieved such prominence.

[29] Ibid., esp. 86–87. Krasner made a similar argument in *Defending the National Interest: Raw Materials Investments and U.S. Foreign Policy* (Princeton: Princeton University Press, 1978).

[30] Gourevitch, "The Second Image Reversed: The International Sources of Domestic Politics," *International Organization* 32 (Autumn 1978), 900. See also idem, *Politics in Hard Times* (Ithaca, N.Y.: Cornell University Press, 1986). Gourevitch's arguments about the impact of the world political economy on domestic coalitions first appeared in his "International Trade, Domestic Coalitions, and Liberty: Comparative Responses to the Crisis of 1873–96," *Journal of Interdisciplinary History* 8 (Autumn 1977).

[31] Outstanding works include David R. Cameron, "The Expansion of the Public Economy: A Comparative Analysis," *American Political Science Review* 72 (December 1978); Peter J. Katzenstein, *Small States in World Markets: Industrial Policy in Europe* (Ithaca, N.Y.: Cornell University Press, 1985); Helen V. Milner, *Resisting Protectionism: Global Industries and the Politics of International Trade* (Princeton: Princeton University Press, 1988); Ronald Rogowski, *Commerce and Coalitions: How Trade Affects Domestic Political Alignments* (Princeton: Princeton University Press, 1989); and Beth A. Simmons, *Who Adjusts? Domestic Sources of Foreign Economic Policy during the Interwar Years* (Princeton: Princeton University Press, 1994).

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We first need to understand the personal and sociological context in which the article was written. In the early 1970s, as Krasner noted in his article, liberal thinking was on the rise. Classical realism, which had dominated international relations theory since World War II, had not equipped political scientists to understand how changes in the world economy were affecting the policies and interactions of states. The fact that Henry Kissinger, a leading figure in the realist camp during the 1950s and early 1960s and Nixon's national security adviser, did not even attend the Camp David meeting (August 13–15, 1971) on closing the gold window illustrates how out of touch classical realism was with the close linkages between wealth and power. Policy-oriented economists and business school professors moved first to fill the vacuum, notably Richard Cooper (then of Yale), in *The Economics of Interdependence* (1968), Charles P. Kindleberger of MIT, in *American Business Abroad* (1969), and Raymond Vernon of Harvard, with *Sovereignty at Bay* (1971).[32] In an article published in this journal in 1972, Cooper summarized the liberal agenda, arguing that "the model regime [of] autonomous and purposeful nation-states in harmonious and unrestricted economic intercourse, through the competitive market place, at fixed exchange rates, governed by occasional treaties and conventions to assure good conduct and to iron out modest problems of overlapping jurisdiction . . . is simply not viable in the long run." A "new conception of world economic order" was needed that would take into account issues of political economy, often dismissed as "low politics."[33]

These ideas resonated at the Center for International Affairs at Harvard, where Joseph S. Nye took the lead in organizing a project on transnational relations. The resulting volume, *Transnational Relations and World Politics*, argued that "high and low politics have become tightly intertwined."[34] After sitting quietly through many seminars on this subject, Samuel P. Huntington of Harvard drew attention to some of the political and strategic dimensions of transnational relations, although, like Krasner, he was skeptical of the liberal premises underlying the Keohane-Nye analysis: "Internationalism is a dead end. Only organizations that are disinterested in sovereignty can transcend it. For the immediate future a central focus of world politics will be on the coexis-

[32] Cooper, *The Economics of Interdependence: Economic Policy in the Atlantic Community* (New York: McGraw Hill for the Council on Foreign Relations, 1968); Kindleberger (fn. 2); Vernon, *Sovereignty at Bay* (New York: Basic Books, 1971).

[33] Richard N. Cooper, "Economic Interdependence and Foreign Policy in the Seventies," *World Politics* 24 (January 1972).

[34] Keohane and Nye (fn. 10), 379.

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tence of and interaction between transnational organizations and the nation-state."[35]

Into this scene stepped Stephen Krasner, first as a graduate student (Ph.D. 1972) writing a dissertation on the political economy of coffee under Huntington's direction, and then as an assistant professor (1971–75). Krasner had already written a biting critique, subtitled "Allison Wonderland," of Graham Allison's famous book on the Cuban Missile Crisis.[36] "State Power" was his attempt to challenge the liberal transnational research agenda—which played such a key role in discourse at the Center for International Affairs—by pointing out the shallowness of its political roots. It was also his "farewell address" to Harvard, written during the year in which he moved to UCLA.

But why did "State Power" become such a focus of debate? Perhaps the word "focus" provides a clue: in game-theoretic language, "State Power" served as a focal point in a coordination game. The three major constituencies of IPE at the time—liberal transnationalists, statist realists, and their audiences of confused or bemused students, faculty, and policymakers—all benefited from Krasner's definition of the issues. Writers, teachers, and students seized on his argument about hegemony and openness. The fact that Krasner explicitly gave priority to political factors helped all political scientists lay claim to this "turf." Realists, seeking to revise their doctrine to take account of political economy, benefited from having Krasner as a champion; liberals found that he had posed questions that they could seek to answer differently.[37] Students clarified their understanding of the issues by reading works that directly challenged one another. Once attention was focused on the problem as Krasner had posed it, authors had incentives to join in the debate and share in the academic notoriety. The drama of a good controversy, while potentially misleading, can make for exciting teaching. It also sells books and builds reputations.

The focal point effect was reinforced by an extraordinary collection of young international relations faculty and graduate students concentrated at Harvard in the early 1970s. Krasner sat around the seminar and lunch tables at the CFIA (then housed in a small building on Divinity Avenue) with Peter Gourevitch, Robert Jervis, Peter Katzenstein, James Kurth, Joseph Nye, Samuel Popkin, and Martin Shefter. The

[35] Huntington, "Transnational Organizations in World Politics," *World Politics* 25 (April 1973), 368.

[36] Krasner, "Are Bureaucracies Important?" *Foreign Policy*, no. 7 (1972), commenting on Graham T. Allison, *Essence of Decision: Explaining the Cuban Missile Crisis* (Boston: Little, Brown, 1971). Allison was also at Harvard.

[37] As I sought to do in "The Theory of Hegemonic Stability" (fn. 13).

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usual Harvard hierarchy was absent because the senior faculty did not dominate these discussions: argument was intense, continuing, and among friends.[38] Ideas were generated and refined, and when several participants soon migrated to UCLA, San Diego, Swarthmore, or Cornell, they took their ideas with them. The "stars were aligned" in a rare conjuncture of ideas, institutions, and individuals.

VI. Puzzles and Propositions in Contemporary Political Economy

Krasner identified a salient puzzle: changing patterns of openness and closure that did not lend themselves to explanation by neoclassical trade theory. His central argument was based on Albert Hirschman's insight that trade and investment patterns have power implications. Since power is a major goal of states, Krasner argued, the implications of openness for state power could be a key to unlocking the puzzle of variation in policies toward openness.

Students of the world political economy face a different puzzle now: how to account for the "rush to openness" on the part of a wide variety of states. This wave of liberalization includes states formerly part of the Soviet empire, but it is evident worldwide. After decades of devotion to state-led protectionism, Mexico after the mid-1980s joined the GATT, then entered into a free-trade arrangement with the United States and Canada. China, cited by Krasner as a classic example of a large, less developed, closed economy, now runs the world's largest trade surplus with the United States and is demanding entry into the World Trade Organization. Ironically, it seems, no sooner did political scientists gain some understanding of why, despite neoclassical economics, economic liberalization is not universal than it became more universal than ever!

What is the key, comparable to the Hirschman-Krasner insight about trade and power, that can unlock this puzzle? One candidate for the honors is that bête noire of political science, open-economy macroeconomics itself. Jeffry Frieden and Ronald Rogowski have recently argued that the key explanatory factor is a long-term decrease in the costs of international exchange and the concomitant increases in the rewards. These changes in costs and benefits increase the incentives for governments to liberalize trade and payments, even when the economy begins from a position of substantial closure. Openness is self-reinforcing within countries, since it increases the economic strength and political

[38] As Stephen Krasner recalls, Joseph Nye was a recently tenured member of the Harvard faculty at that time but did not stifle discussion.

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influence of interests that favor it; it is contagious, because the openness of others creates more opportunities for gains from exchange.[39]

Whereas Krasner emphasized how considerations of power overcome purely economic incentives, Frieden and Rogowski argue that economics trumps politics, that the state is little more than a cash register that adds up the demands of interest groups in a democratic polity. Such a state presupposes accountability of the ruling elites to social groups. As the authors observe, "Whatever attunes policymakers to broad social interests or gives them longer time horizons will make them likelier to internalize the benefits of increased international trade."[40] Such a simple view of interest-group politics is unlikely to be attractive to most students of comparative politics. Like Krasner's argument, it is both lucid and problematic.[41] Those of us who believe in the importance of institutions may be inclined to sniff, "disconfirmed," and turn our attention elsewhere. However, the history of scholarship related to Krasner's "State Power" article suggests that such a move might be a mistake. Open-economy theory, based on macroeconomics, is simplistic, but it has a propositional clarity lacking in interpretations fraught with caveats. It would not be surprising to see it generate a countermovement from institutionalists, forcing them to devise more rigorous specifications of the conditions under which domestic institutions will block price signals, freeze coalitions and policies, or channel responses to change in ways that cannot be predicted from economic structure alone. Indeed, recent work by Geoffrey Garrett and Peter Lange takes a significant step in this direction.[42] The dialogue between these two sets of arguments may be productive, as was the dialogue between those, like Krasner, who focused on international structure and those, like Gourevitch and Katzenstein, who examined domestic politics.

A more political account of the rush to liberalization could begin by setting aside the notion of an undifferentiated state—a weak point in Krasner's argument—and focusing instead on the interests of key elites. In many of the states pursuing policies of economic liberalization, these elites are not institutionally accountable to society in any real sense. Indeed, control of state power, as in Russia, may lead to control of eco-

[39] Frieden and Rogowski, "The Impact of the International Economy on National Policies: An Analytical Overview," in Robert O. Keohane and Helen V. Milner, eds., *Internationalization and Domestic Politics* (Cambridge: Cambridge University Press, 1996).

[40] Ibid., 35.

[41] See especially the chapters in Keohane and Milner (fn. 39) on Europe by Geoffrey Garrett, on Japan by Frances Rosenbluth, on the Soviet Union and Russia by Matthew Evangelista, and on several developing countries by Stephan Haggard and Sylvia Maxfield.

[42] Garrett and Lange, "Internationalization, Institutions, and Political Change," in Keohane and Milner (fn. 39).

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nomic assets, rather than vice versa. Such societies, even if nominally democratic, are more aptly designated "kleptocracies"—polities ruled by thieves; their rulers are predatory in the sense used by Margaret Levi.[43] Yet since they control state organizations and can claim legitimacy as sovereign representatives, they can borrow money from international financial institutions, negotiate trade agreements, and impose rules for acquiring state assets that discriminate in favor of themselves and their business partners. Perhaps one key to global liberalization should be sought, not in governments' conversion to superior economic doctrine nor in pressure on them by interest groups to capture potential gains from exchange, but in the recognition by predatory rulers that they can manipulate the terms of international exchange. Positioned at the nexus of the domestic and global political economies, such rulers control profitable international connections and skim off profits from the world economy without having to tax their subjects directly.

The emerging dialogue between economistic theories of liberalization and more political approaches may or may not turn out to be as productive as the strands of thought and argument that emanated from hegemony theory, and especially from Stephen Krasner's formulation of it. As in the past, however, the study of world politics is likely in the future to progress less through incremental additions to our knowledge than through leaps of imagination and the critical, dialectical discourses that these flights of fancy generate. Stephen D. Krasner took such a leap in "State Power and the Structure of International Trade." We understand the relationship between international political structure and economic openness much better than we did before the appearance of this article and the stream of research that it helped to generate. Students of international political economy may remain dissatisfied with the answers we have generated, but without the lucidity of Krasner's formulation, we would surely be more confused than we are.

[43] Levi, *Of Rule and Revenue* (Berkeley: University of California Press, 1988), esp. chap. 2, "The Theory of Predatory Rule."

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THE SECURITY DILEMMA REVISITED

By CHARLES L. GLASER*

ROBERT Jervis's article "Cooperation under the Security Dilemma" is among the most important works in international relations of the past few decades. In it, Jervis develops two essential arguments. First, he explains that the security dilemma is the key to understanding how in an anarchic international system states with fundamentally compatible goals still end up in competition and at war. The security dilemma exists when "many of the means by which a state tries to increase its security decrease the security of others."[1] It provides the rational foundation for what Jervis termed the "spiral model," which describes how the interaction between states that are seeking only security can fuel competition and strain political relations.[2] Second, Jervis explains that the magnitude and nature of the security dilemma depend on two variables: the offense-defense balance and offense-defense differentiation.[3] As a result, the security dilemma can vary across space and time. Although states exist in a condition of international anarchy that does not vary, there can be significant variation in the attractiveness of cooperative or competitive means, the prospects for achieving a high level of security, and the probability of war.

* For comments on earlier drafts I would like to thank Lynn Eden, Colin Elman, Matt Evangelista, Jim Fearon, Lloyd Gruber, Chaim Kaufmann, Andy Kydd, Joseph Lepgold, Jim Morrow, and Steve Walt.

[1] Robert Jervis, "Cooperation under the Security Dilemma," *World Politics* 30 (January 1978), 169. For earlier discussions of the security dilemma, see John H. Herz, "Idealist Internationalism and the Security Dilemma," *World Politics* 2 (January 1950), which identifies the basic concept, but does not develop it extensively; and Herbert Butterfield, *History and Human Relations* (London: Collins, 1951).

[2] Robert Jervis, *Perception and Misperception in International Politics* (Princeton: Princeton University Press, 1976), chap. 3, esp. 62–76; these pages provide a more thorough discussion of the basic workings of the security dilemma than does "Cooperation under the Security Dilemma." For an early discussion of this type of interaction, see J. David Singer, "Threat-Perception and the Armament-Tension Dilemma," *Journal of Conflict Resolution* 2 (March 1958); for a recent discussion, see Charles L. Glaser, "Political Consequences of Military Strategy: Expanding and Refining the Spiral and Deterrence Models," *World Politics* 44 (July 1992).

[3] Many similar arguments are developed by George Quester, *Offense and Defense in the International System* (New York: Wiley, 1977). See also Marion William Boggs, *Attempts to Define and Limit "Aggressive" Armament and Strategy* (Columbia: University of Missouri, 1941); and Marlies Ter Borg, "Reducing Offensive Capabilities: The Attempt of 1932," *Journal of Peace Research* 29, no. 2 (1992).

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These arguments laid the foundation for a major debate within realism, which is widely considered the dominant paradigm in international relations. As Jervis's analysis rests on the same basic assumptions—anarchy and fundamental compatibility of goals—that underlie structural realism, it should be viewed as part of this overall tradition. The broad implications of his argument are dramatically different from the standard structural-realist analysis, however, since the latter does not focus on the security dilemma and consequently envisions a consistently more competitive and dangerous world.[4] Although the debate over these formulations of structural realism has evolved since publication of Jervis's article, the matter is yet to be resolved.[5]

We can also appreciate the importance and impact of security dilemma and offense-defense arguments by recognizing that scholars have now employed these arguments effectively to address many of the most important questions of international relations theory and security policy, including the effectiveness of deterrence and reassurance,[6] sources of moderation in Soviet policy,[7] the severity of relative gains constraints,[8] alliance behavior,[9] military doctrine,[10] imperial expan-

[4] The major statement of the standard structural-realist analysis is Kenneth N. Waltz, *Theory of International Politics* (Reading, Mass.: Addison-Wesley, 1979); also important are idem, *Man, the State and War* (New York: Columbia University Press, 1959), esp. chaps. 6, 7; and idem, "Reflections on *Theory of International Politics*: A Response to My Critics," in Robert O. Keohane, ed., *Neorealism and Its Critics* (New York: Columbia University Press, 1986).

[5] See Charles L. Glaser, "Realists as Optimists: Cooperation as Self-Help," *International Security* 19 (Winter 1994–95); and Stephen Van Evera, *Causes of War*, vol. 1, *The Structure of Power and the Roots of War* (Ithaca, N.Y.: Cornell University Press, forthcoming).

[6] Although not framed in terms of the security dilemma, see, for example, Richard Ned Lebow and Janice Gross Stein, "Beyond Deterrence," *Journal of Social Issues* 43 (Winter 1987). See also Stein, "Deterrence and Reassurance," in Philip E. Tetlock et al., *Behavior, Society and Nuclear War*, vol. 2 (New York: Oxford University Press, 1991), esp. 17; this essay gives somewhat greater prominence to the security dilemma. On the more general question of resolving political rivalries, see Sean Lynn-Jones, "Rivalry and Rapprochement: Accommodation between Adversaries in International Politics" (Ph.D. diss., Harvard University, in process).

[7] Matthew Evangelista, "Sources of Moderation in Soviet Security Policy," in Tetlock et al. (fn. 6), esp. 290–96; and idem, "Cooperation Theory and Disarmament Negotiations in the 1950s," *World Politics* 42 (July 1990). On the Soviet use of concessions, see Deborah Welch Larson, "Crisis Prevention and the Austria State Treaty," *International Organization* 41 (Winter 1987).

[8] Robert Powell, "Absolute and Relative Gains in International Relations Theory," *American Political Science Review* 85 (December 1991); Powell casts the argument in terms of the costs of war, not in terms of the offense-defense balance. And see Glaser (fn. 5), 79.

[9] On balancing versus bandwagoning, see Stephen M. Walt, *The Origins of Alliances* (Ithaca, N.Y.: Cornell University Press, 1987), esp. 24–25, 165–67. On the tightness of alliances, see Thomas J. Christensen and Jack Snyder, "Chain Gangs and Passed Bucks: Predicting Alliance Patterns in Multipolarity," *International Organization* 44 (Spring 1990); and Thomas J. Christensen, "Perceptions and Allies in Europe, 1865–1940," *International Organization* 51 (Winter 1997); disagreeing is James D. Morrow, "Arms versus Allies: Trade-offs in the Search for Security," *International Organization* 47 (Spring 1993). On the alliance choices of small powers, see Karl Mueller, "Patterns of Alliance: Alignment Balancing and Stability in Eastern Europe," *Security Studies* 5 (Autumn 1995).

[10] Barry R. Posen, *The Sources of Military Doctrine: France, Britain and Germany between the World Wars* (Ithaca, N.Y.: Cornell University Press, 1984), esp. 67, 74, 221–22, 236–39; Jack Snyder, *The*

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sion,[11] revolution and war,[12] ethnic conflict,[13] conventional arms control,[14] U.S. nuclear policy and arms control,[15] nuclear proliferation,[16] the escalatory dangers of conventional war,[17] U.S. grand strategy,[18] and the prospects for peace in Europe and policies for preserving it.[19] Limitations of space unfortunately preclude an examination here of the ways in which security dilemma and offense-defense arguments are used in this literature.

The first sections of this article recapitulate Jervis's basic arguments, discuss work that has added to these arguments, and offer clarifications and further extensions. Although the security dilemma is referred to quite frequently, relatively little effort has been devoted to examining its core logic, some of which was left incomplete by Jervis himself. The most important gaps concern whether and how the security dilemma operates between rational actors. Consequently, I explore three ways in which a state's efforts to increase its security when facing a security dilemma can, without states suffering misperceptions, generate undesirable outcomes.

Ideology of the Offensive (Ithaca, N.Y.: Cornell University Press, 1984); for disagreements, see Scott D. Sagan, "1914 Revisited: Allies, Offense and Instability," *International Security* 11 (Fall 1986); the correspondence between Sagan and Snyder, *International Security* 11 (Winter 1986–87); and Elizabeth Kier, "Culture and Military Doctrine: France between the Wars," *International Security* 19 (Spring 1995).

[11] Jack Snyder, *Myths of Empire: Domestic Politics and International Ambition* (Ithaca, N.Y.: Cornell University Press, 1991), 21–26.

[12] Stephen M. Walt, *Revolutions and War* (Ithaca, N.Y.: Cornell University Press, 1996), esp. 33–45, 334–38.

[13] Barry R. Posen, "The Security Dilemma in Ethnic Conflict," *Survival* 35 (Spring 1993); and Chaim Kaufmann, "Possible and Impossible Solutions to Ethnic Civil Wars," *International Security* 20 (Spring 1996).

[14] Jack Snyder, "Limiting Offensive Conventional Forces: Soviet Proposals and Western Options," *International Security* 12 (Spring 1988), esp. 67–71; see also Stephen Duane Biddle, "The Determinants of Offensiveness and Defensiveness in Conventional Land Warfare" (Ph.D. diss., Harvard University, 1992). Following the basic approach of offense-defense theory, although apparently not directly influenced by it, is the nonprovocative defense literature. See, for example, the special issue of the *Bulletin of the Atomic Scientists* 44 (September 1988); and Anders Boserup and Robert Neild, *The Foundations of Defensive Defense* (New York: St. Martin's, 1990).

[15] Charles L. Glaser, *Analyzing Strategic Nuclear Policy* (Princeton: Princeton University Press, 1990).

[16] Shai Feldman, *Israeli Nuclear Deterrence: A Strategy for the 1980s* (New York: Columbia University Press, 1982).

[17] Barry R. Posen, "Inadvertent Nuclear War? Escalation and NATO's Northern Flank," *International Security* 7 (Fall 1982); and idem, *Inadvertent Escalation: Conventional War and Nuclear Risks* (Ithaca, N.Y.: Cornell University Press, 1992). Offering a different explanation is Jeffrey W. Legro, "Military Culture and Inadvertent Escalation in World War II," *International Security* 18 (Spring 1994).

[18] Stephen M. Walt, "The Case for Finite Containment: Analyzing U.S. Grand Strategy," *International Security* 14 (Summer 1989), esp. 22–30.

[19] Stephen Van Evera, "Primed for Peace: Europe after the Cold War," *International Security* 15 (Winter 1990–91), esp. 11–17; Ted Hopf, "Managing Soviet Disintegration: A Demand for Behavioral Regimes," *International Security* 17 (Summer 1992); Charles A. Kupchan and Clifford A. Kupchan, "Concerts, Collective Security, and the Future of Europe," *International Security* 16 (Summer 1991), esp. 133–37; and Charles L. Glaser, "Why NATO Is Still Best: Future Security Structures for Europe," *International Security* 18 (Summer 1993), esp. 26–33, 38–47.

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The following section argues that two additional variables—the extent of the adversary's greed (that is, motives beyond security) and of the adversary's unit-level knowledge of the state's motives—influence the magnitude of the security dilemma. Thus, whether a theory posits only security seekers or instead posits some greedy states is a pivotal choice. In the latter case, the role of the security dilemma is diminished and competitive policies are more likely to avoid conflict. Whether states can rely on unit-level information about others' motives can have equally important implications, enabling a state to be secure when it would otherwise be insecure, which in some cases supports more cooperative policies and in other cases more competitive ones. By considering these variables, one also integrates the security dilemma into broader debates over international relations theory and security policy. These variables, for example, establish the divide between the spiral model and what Jervis termed the "deterrence model," which applies to secure greedy states and therefore rejects the security dilemma.

The final section addresses basic criticisms of the security dilemma and offense-defense theory, including (1) the empirical claim that greedy states, not the security dilemma, are the main source of international conflict; (2) the security dilemma does not really exist, because its internal logic is flawed, or because its constraints are always overwhelmed by other considerations, or because states construct the security dilemma and therefore can choose not to; and (3) offense-defense theory is flawed. I argue that the greedy-states criticism poses a serious challenge but that the others are based on incomplete or flawed analysis.

The Security Dilemma: How Does It Lead to Undesirable Outcomes?

Jervis defines the security dilemma as a situation in which "the means by which a state tries to increase its security decrease the security of others." This definition by itself does not make clear why the security dilemma is a problem, however: if states value their own security but not the security of others, why would an action that makes one's adversary less secure necessarily be bad? The most obvious reason is that the adversary is likely to react to having its security reduced. And by the same logic the adversary's reaction will in turn reduce the state's security. But why does not this action-reaction process simply leave the state's security unchanged, since the adversary's reaction could just offset the state's action?

This section identifies three distinct ways in which making one's adversary less secure can be self-defeating, leaving the state worse off than

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before its initial action: (1) by setting in motion a process that reduces the state's own military capability, that is, its ability to perform military missions; (2) by increasing the value the adversary places on expansion, which makes it harder to deter; and (3) by simply wasting money. Although Jervis touches on each of these possibilities, he does not fully explain how the first two could happen without misperceptions. Thus, although he stresses that the "heart of the security dilemma" stems from "the anarchic context of international relations," Jervis leaves some key arguments underdeveloped.[20] The following discussion is intended to close some of these gaps. This section ends with a brief discussion of why states sometimes cannot avoid these undesirable outcomes.

By further developing these arguments I do not mean to imply that misperceptions are unimportant. Whether or not states suffer from significant misperceptions, we need to analyze how a rational state would act if we are to determine how much of a state's insecurity is really the product of the conditions it faces and how much worse relations become when misperceptions do occur.

Reducing the State's Military Capability

The idea that arming could reduce capabilities might seem self-contradictory, but the issue is one of terminology. I use "military capability" to refer specifically to the state's ability to perform military missions, not to the size of its forces or its total military assets.[21] Thus, an action-reaction process will leave the state with more military assets, but it may nevertheless reduce its military capability vis-à-vis its adversary.

Because of the security dilemma, when the state arms, it makes its adversary less secure by reducing the adversary's ability to defend itself. The adversary then buys additional arms in order to restore its military capability.[22] At first glance, it might appear that the net effect of this action-reaction process would be to leave both countries' military capabilities unchanged, since each country's additional forces would simply offset the other's.[23]

[20] Jervis (fn. 2), 76.

[21] On the advantages of this usage of "capability," see Glenn H. Snyder, "Process Variables in Neorealist Theory," *Security Studies* 5 (Spring 1996), 180–83.

[22] For the sake of simplicity, I will focus on the decision to buy arms. However, the logic of the security dilemma is more general, including the decision to take territory and to acquire allies to increase security. Regarding territory, an action-reaction process could be expansion into part of a buffer zone that leads one's adversary to expand into the remainder of the zone.

[23] Jervis (fn. 2), 64, says that such an action-reaction process results in reduced security because "when states seek the ability to defend themselves, they get too much and too little . . . too little because others, being menaced, will increase their own arms and so reduce the first state's security." However, this explains only why the state's security is reduced relative to the situation following its initial buildup, but not why it should be reduced relative to the prior military status quo.

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In fact, however, when the action-reaction process shifts the offense-defense balance, the result is instead a change—a decrease or an increase—in military capabilities. If the state deploys a new weapons system that favors offensive missions and if its adversary responds by deploying this system, the country's ability to defend itself will be reduced, leaving it less secure than before this round of arming. MIRVed missiles are usually considered to be this type of offensive innovation, reducing the ease with which the United States and the Soviet Union could meet their requirements for deterrence. If, however, the state deploys an innovation that favors defensive missions and its adversary matches it, then the net result would be an increase in the state's capability to defend itself and an increase in its security. The precision-guided munitions (PGMs) deployed on the Central Front during the cold war appear to have been such an innovation.[24]

The ability to perform offensive and defensive missions can also vary with force size.[25] Thus, action-reaction processes that result in larger forces (as distinguished from different types of forces) can increase or decrease the state's military capability for defense. For example, equal increases in the size of conventional ground forces can result in an increase in a state's ability to defend, by enabling it to increase the density of forces deployed along the front. Similarly, equal increases in the size of nuclear forces can increase both countries' retaliatory capabilities, thereby enhancing their deterrent capabilities. In such cases, an action-reaction process increases security.

Appreciation of the possibility that arms competition can reduce both countries' military capabilities precedes Jervis's work on the security dilemma. In fact, the complementary observation that adversaries can have a mutual interest in reciprocating arms restraint is one of the core insights of modern arms control theory, which was developed in the late 1950s and the early 1960s.[26] Setting this insight within the broader framework of the security dilemma, however, highlights an important issue that arms control theory tends to gloss over—the compatibility of the intrinsic goals of the states involved. The security dilemma makes explicit the possibility that both states in the competition are interested only in security, and it deepens our understanding of how this compe-

[24] John J. Mearsheimer, *Conventional Deterrence* (Ithaca, N.Y.: Cornell University Press, 1983), chap. 7.

[25] This possibility is discussed in Chaim Kaufmann and Charles Glaser, "What Is the Offense-Defense Balance and Can We Measure It?" *International Security* (forthcoming).

[26] See, for example, Thomas C. Schelling and Morton H. Halperin, *Strategy and Arms Control* (New York: Twentieth Century Fund, 1961), esp. 1–2.

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tition can arise. In addition, Jervis lays the groundwork for more recent work by framing the problem of cooperation in terms of Prisoners' Dilemma and Stag Hunt game-theory matrices and exploring how the prospects for cooperation vary with changes in the relative size of countries' payoffs (pp. 170–83).[27] In more recent years, cooperation theorists have used game theory to establish a more rigorous foundation for the core findings of arms control theory; they frequently use the repeated Prisoners' Dilemma to model arms competition.[28]

Making One's Adversary Less Secure, Which Increases the Value It Places on Expansion

THE DANGER OF THE ADVERSARY'S INSECURITY

Reducing an adversary's security can reduce the state's own security in a second way—by increasing the value the adversary places on expansion, thereby making it harder to deter. Making an adversary more insecure will often increase its interest in expansion, since expansion can often increase security. For example, a more insecure adversary will find expansion more desirable when it can provide more secure borders, strategic depth, or control of resources that are valuable for building military capabilities.[29]

Consequently, even when arming increases a state's military capability, the net result could be a reduction in its security. On the one hand, the state will enjoy the enhanced deterrent and defense capabilities provided by its improved military capability. On the other hand, because the adversary is now harder to deter, it may not be deterred by these enhanced capabilities, even if it would previously have been deterred by less effective military capabilities.

Thus, states that can achieve military advantages should not always seek them. There is no general answer to whether sustainable military

[27] For earlier use of game theory to explore the different motives that can lead to arms competition and cooperation, see Thomas C. Schelling, "A Framework for the Evaluation of Arms-Control Proposals," *Daedulus* 104 (Summer 1975). While remaining positive about the potential contribution of game-theoretic formulations, Jervis explores their shortcomings in "Realism, Game Theory, and Cooperation," *World Politics* 40 (April 1988).

[28] Robert Axelrod, *The Evolution of Cooperation* (New York: Basic Books, 1984). For a focus on questions of arming, see George W. Downs, David M. Rocke, and Randolph M. Siverson, "Arms Races and Cooperation," in Kenneth A. Oye, ed., *Cooperation under Anarchy* (Princeton: Princeton University Press, 1986).

[29] Jervis (fn. 1), 168–69. Although Jervis identifies this phenomenon as being separate from the security dilemma, I describe it as an integral part of the security dilemma. On the resource value of territory, see Peter Liberman, "The Spoils of Conquest," *International Security* 18 (Fall 1993); and idem, *Does Conquest Pay? The Exploitation of Occupied Industrial Societies* (Princeton: Princeton University Press, 1996).

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advantages that leave one's adversary less secure will increase or decrease the state's security, but policy analyses that grapple with this trade-off find that sometimes cooperation or restraint is preferable to more competitive policies. In the current debate over NATO expansion, for example, opponents argue that even though expansion into Central Europe would increase NATO's military capability, it would also increase Russian insecurity and therefore make Russian invasion of its neighbors more likely.[30]

ACTIONS THAT REDUCE AN ADVERSARY'S SECURITY: SIGNALING OF MOTIVES

A state's military buildup can make its adversary less secure in two ways. First, and more straightforward, the state's buildup can reduce the adversary's ability to defend itself. Even if the adversary matches the state's initial buildup, the net result could be a reduction in its military capability, as described above. It is also possible that the adversary will end up less capable of defense because it is unable to match or counter the state's buildup.

Second, a state's military buildup can change the adversary's beliefs about the state's motives, convincing the adversary that the state is inherently more dangerous than it had previously believed. More specifically, the state's buildup could increase the adversary's assessment of the extent to which it is motivated by the desire to expand for reasons other than security,[31] which I will term greed.[32] This type of action-reaction process is the focus of the spiral model, in which countries that are seeking only security conclude that their adversary's motives are more malign, that is, greedier, than previously believed.[33] Although misperceptions can make spirals more intense, Jervis, as already noted, stresses that both the security dilemma and the spiral model have a rational foundation.[34]

[30] See, for example, Michael E. Brown, "The Flawed Logic of NATO Expansion," *Survival* 37 (Spring 1995). For a security dilemma–based argument against nuclear superiority, see Glaser (fn. 15), chap. 5.

[31] The adversary could also become less secure if it concludes that the state places a higher value on security or demands a higher level of security, both of which could make the state harder to deter. For simplicity, I focus on the adversary's assessment of the state's greed.

[32] I use the term "greedy" because states can be motivated to expand for two fundamentally different types of reasons—security and greed—which are blurred by the more common terms "expansionist" and "aggressive." Four types of states can be defined in terms of greed and security seeking; see Glaser (fn. 2), 501–3. In referring to states as greedy, I do not mean to imply that they do not also seek security.

[33] Jervis (fn. 2), chap. 3, esp. 62–76. See also Glenn Snyder, "The Security Dilemma in Alliance Politics," *World Politics* 36 (July 1984), 468–70; Snyder argues that firm alliance policies can generate reactions that are comparable to a spiral generated by an arms buildup.

[34] In addition to the passage cited in fn. 20, see, for example, Jervis (fn. 2), 62.

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Nonetheless, the possibility of a rational spiral presents something of a puzzle. How could the arms policy of a rational state that seeks only security convince its adversary, based on logically sound inferences, that the state is more dangerous than was previously believed? The answer is not obvious because, for the following reasons, the adversary will understand that an arms buildup may be motivated by security, not greed. Recognizing that the state does not know its motives, the adversary will appreciate the state's desire for adequate defense capabilities, which could require more or improved armed forces. In addition, appreciating the security dilemma, the adversary will understand that forces that the state requires for increased security could reduce its own security. A rational adversary will therefore have reasons to expect a pure security seeker to engage in a threatening arms buildup and consequently will not automatically conclude that such a buildup reflects greedy motives.

The question then is, when does a state's military buildup signal greedy motives? A common, although incorrect, claim is that structural theories do not allow states to know anything about the motives of others, since they are unobservable. But this claim overlooks the possibility that certain actions can communicate valuable information because they are not equally likely to be taken by a greedy state and a pure security seeker. Therefore, for example, when a state launches a military buildup that is more likely to be taken by a greedy state than by a pure security seeker, an adversary that is making sound inferences will update its assessment of the state's motives, concluding that the state is more likely than previously believed to be greedy.[35]

Two types of military buildups could help with this kind of differentiation. In the first type, different types of states prefer different size forces: a greedy state is more likely than a security seeker to add forces beyond those required for adequate defense of its territory. Even though extra forces would provide some additional capability to defend, a state interested only in security would see less value in these forces than would a greedy state and therefore would be less willing to pay for them. In the second type of buildup, states prefer different types of forces. For example, when a state has a choice between forces that add roughly equally to offensive military missions and defensive ones, a greedy state is more likely than a security seeker to choose the type of

[35] On signaling, see Robert Jervis, *The Logic of Images in International Relations* (Princeton: Princeton University Press, 1970); and James D. Fearon, "Threats to Use Force: The Role of Costly Signals in International Crises" (Ph.D. diss., University of California, Berkeley, 1992).

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forces that improve its offense.[36] The greedy state sees both greater value in offense and sometimes less value in not provoking others, since it anticipates conflict anyway.

The question that remains is, can an adversary that makes logically sound inferences nevertheless reach incorrect conclusions about the state's goals?[37] For the interaction between pure security seekers to produce the increasing fear and insecurity explained by a rational spiral model, the answer must be yes. Otherwise, misperceptions would be required to generate a spiral.

This type of interaction can occur if the states are uncertain about the size or type of forces required to maintain a given level of security. Given this uncertainty, consider the simple case in which some pure security seekers would be satisfied with a lower level of forces and some with a higher level, but all greedy states would require a higher level. A pure security seeker that builds to the higher level will then convince its adversary that it is more likely to be greedy, since only some security seekers but all greedy states would build to this level. Similar interactions can occur if there is uncertainty about whether security seekers require offensive capabilities and if there is uncertainty about the level of security that security seekers believe is adequate.[38]

A spiral can also result from an adversary's uncertainty about the state's understanding of its motives. For example, when the adversary believes that the state believes there is only a small probability that the adversary is greedy and, therefore, that the state does not fear it, the adversary will conclude that the state's buildup is largely unnecessary for security and therefore that the state is likely to be motivated by greed. If the adversary's initial estimate of the probability that the state is fearful is too low, then this increase in its assessment that the state is greedy will be too large, resulting in a spiral. Kydd's formal analysis of this interaction shows that under a wide range of conditions a rational adversary will find the state's buildup to be provocative and that updating of beliefs is sensitive to prior expectations about the state's motives.[39]

[36] This is an oversimplification, however, since a pure security seeker might want the capability to take territory for a variety of reasons. See fn. 57 below for qualifications.

[37] Although Jervis identifies the types of buildups, he does not really explain how rational states can spiral; Jervis (fn. 1), 199–201. He notes that states often cannot accurately infer motives from others' military forces and therefore they tend to assume the worst; Jervis (fn. 2), 64–65. This would clearly generate increased hostility, but the judgment is logically flawed, since without additional information states' assessments should remain unchanged. Moreover, states should not assume the worst in the face of uncertainty, since this can support policies that are too competitive/provocative.

[38] For discussion of related issues concerning subjective security requirements, see Jervis (fn. 1), 174–76.

[39] Andrew Kydd, "Game Theory and the Spiral Model," *World Politics* 49 (April 1997). Kydd notes (p. 373) that Jervis explores this dynamic for the extreme cases in which bias leads the adversary to

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The logic of these signaling arguments also works in the opposite direction—a state can sometimes use restraint in building military forces to reduce the adversary's concern about its greediness.[40] A greedy state wants to mislead its adversaries into believing that it is interested only in security, since its adversaries would then be more likely to pursue policies that leave them vulnerable. Given these incentives for a greedy state to misrepresent its motives, a security seeker can communicate information about its motives only by adopting a policy that would be less costly for it than for a greedy state.

Depending on the conditions they face, states can try to communicate their benign intentions via three types of military policies.[41] First, arms control agreements that limit both countries' current or future ability to perform offensive missions communicate a lack of greed, since a greedy state sees greater value in offensive missions than does a pure security seeker. Second, a state may be able to adopt unilateral defense, choosing to protect its country with a defensive doctrine, even if its adversary continues to pursue an offensive one. When offense has the advantage, maintaining its security via unilateral defense will require the state to outspend its adversary, which reinforces the message that its motives are benign. Finally, a state can exercise unilateral restraint, that is, reduce its military capability below what it would choose for adequate deterrence and defense were it not considering the effects of signaling. One use of unilateral restraint is primarily tactical—to set in motion a process of reciprocated restraint.[42] Even if not reciprocated, however, unilateral restraint can succeed by communicating that the state is not greedy and is committed to improving relations. Thus, although the state's ability to defend is reduced, the net effect can be an increase in its security. There is, however, the danger that the adversary will misinterpret the state's restraint, seeing a lack of resolve instead of a lack greed; in this case, restraint encourages the adversary to challenge the state. The dual dangers of military shortfalls and underestimates of their resolve make states reluctant to pursue ambitious policies of unilateral restraint.

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assume the state is definitely secure. For pure security seekers, however, all that is necessary to generate a spiral is for the adversary to believe there is some possibility that the state is secure. See also George W. Downs and David M. Rocke, *Tacit Bargaining, Arms Races, and Arms Control* (Ann Arbor: University of Michigan Press, 1990), chap. 4.

[40] Kydd (fn. 39) provides a formal treatment.

[41] Glaser (fn. 5), 67–70; and idem (fn. 2), 526–33; for related points, see Downs, Rocke, and Siverson (fn. 28). On reassurance more generally, see Stein (fn. 6); on the inhibiting effect of appearing weak, see James D. Morrow, "Signaling Difficulties with Linkage in Crisis Bargaining," *International Studies Quarterly* 36 (June 1996).

[42] This approach, often referred to as GRIT, was developed by Charles E. Osgood, *An Alternative to War or Surrender* (Urbana: University of Illinois, 1962).

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MISPERCEPTIONS

In contrast to the rational updating we have considered so far, Jervis also discusses the role of psychological biases in contributing to states' overly hostile assessments of others' actions.[43] More recent work has focused on bureaucratic and domestic political processes as alternative sources of bias.

Statesmen who do not understand the security dilemma and therefore do a poor job of appreciating the choices faced by their adversary will infer incorrectly that the adversary's buildup reflects greedy motives. A state is likely to make errors that build on each other: a key initial mistake is for a state to assume that others know it is interested only in security; the state is then likely to assume that others will not be threatened by its buildup. Consequently, the state is inclined to see the adversary's arms buildup as a sign of greed, when in fact the adversary is building in response to the state's buildup. This is a distorted form of the rational spiral, described above, which is driven entirely by uncertainty about motives; when this bias prevails, states will be more insecure and competition will be more intense than is predicted by a rational security dilemma.

Attribution theory offers a psychological explanation for this type of flawed reasoning.[44] Leaders commonly make the mistake of interpreting the behavior of other countries in terms of their goals/motives, even though they understand their own behavior differently, in terms of the situation they face. As a result, leaders fail to appreciate that other countries face a security dilemma and therefore interpret the actions of others as reflecting greed, even though a pure security seeker might have acted the same way.

Recent work on misperceptions provides alternative explanations, locating these analytic flaws at the level of the state instead of at the level of the individual. This work uses theories of organizational behavior and domestic political dynamics to explain why states often exaggerate an adversary's hostility.[45] Militaries are inclined to exaggerate the offensive potential of the adversary's forces and to impute malign intentions, even when the purposes of the adversary's forces are ambiguous.

[43] Misperception receives far less discussion in "Cooperation under the Security Dilemma" (but see pp. 181–83) than in Jervis (fn. 2), 67–76.

[44] On attribution theory, see Jervis (fn. 2), esp. 35–48; Deborah Welch Larson, *Origins of Containment: A Psychological Explanation* (Princeton: Princeton University Press, 1985), esp. 34–42; and Jonathan Mercer, *Reputation and International Politics* (Ithaca, N.Y.: Cornell University Press, 1996).

[45] For an emphasis on organizational perspectives, see Stephen Van Evera, "Causes of War" (Ph.D. diss., University of California, Berkeley, 1984), pt. 2; and Snyder (fn. 10). Snyder (fn. 11) emphasizes domestic political dynamics.

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Powerful interest groups that would benefit from military competition or expansion often advance self-serving strategic arguments that exaggerate the threat posed by the adversary's capabilities or motives. Furthermore, states are inclined to create myths about the unthreatening nature of their own behavior, which makes adversaries appear still more dangerous.[46]

In sum, through rational updating a state's military buildup (or restraint) can lead an adversary to alter its assessment of the state's motives. Research on misperceptions cautions that states are inclined to do a poor job of updating, with a bias toward exaggerating the hostility of others. When they suffer from these biases, states will act as though the security dilemma is more severe than it actually is.

Simply Wastes Money

The third way in which efforts to make oneself more secure can be self-defeating is by simply wasting money. Unlike the first two cases, the state would not end up less secure for having pursued a policy that initially made its adversary less secure. In this case, the action-reaction process does not reduce the state's military capability; as we have seen, action-reaction processes could leave the state's ability to defend itself unchanged. Nor does the action-reaction generate a spiral of increasingly negative views of the adversary's motives, which is possible since not all military buildups provide information about motives. Nevertheless, the security dilemma results in self-defeating efforts that leave the state less prosperous, yet no more secure.[47]

Why Can't States Cooperate to Avoid Less Desirable Outcomes?

When arming and engaging in competitive policies more generally would be self-defeating, why cannot rational states cooperate to avoid undesirable outcomes? Jervis's formulation of international relations under anarchy as a Stag Hunt raises this question starkly, since cooperation would then be the best option for both countries.[48]

[46] Van Evera (fn. 45), chap. 8; and idem, "Why States Believe Foolish Ideas: Non-Self-Evaluation by Government and Society" (Paper presented at the annual meeting of the American Political Science Association, Washington, D.C., 1988).

[47] Snyder (fn. 33), 461, emphasizes wasted resources; he argues (p. 462) that alliance formation is similar to arming, in that all states would be better off remaining outside an alliance; action and reaction nevertheless generate alliance blocks that are costly but fail to increase security.

[48] On Stag Hunt and related games, see Kenneth A. Oye, "Explaining Cooperation under Anarchy: Hypotheses and Strategies," in Oye (fn. 28).

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The core of the answer lies in appreciating that states are uncertain about their adversaries' motives, lacking confidence that others are pure security seekers. Uncertainty about motives means that states cannot be confident that they are in a Stag Hunt, even if they are. This uncertainty about the type of game can make competition/arming the state's best option. Jervis takes a different view, identifying different sources of competition when in a Stag Hunt—misperceptions and irrational behavior (p. 168). Important as misperceptions and irrational behavior can be, however, these are additions to the basic story.[49] From the perspective of structural realism and the emphasis it places on the implications of anarchy, uncertainty about the adversary—both its motives and its understanding of one's own motives—is the fundamental ingredient for understanding competition.[50] This subsection describes a simple game-theory model of how uncertainty about the adversary could reduce the prospects for productive cooperation.

It is useful to begin by considering why two security seekers might be in a Stag Hunt.[51] On the one hand, a pure security seeker that is confident of not being attacked has no reason to expand: it is satisfied with the territory it controls and does not see instrumental value in additional territory, because it does not fear attack. It could prefer the territorial status quo (CC) to unopposed expansion (DC) for a variety of reasons, including how costly it is to govern the additional territory or because expansion violates an international norm that the state values. Because the state values its own territory, it could prefer fighting to protect its territory (DD) to allowing its adversary to expand unopposed (CD). If two pure security seekers have these preferences and this is common knowledge, then they are in a Stag Hunt and both countries should optimally choose to cooperate.

On the other hand, a state that is unsure whether its adversary is a pure security seeker faces a very different situation. A greedy adversary will prefer unopposed expansion to the territorial status quo; it has Prisoners' Dilemma preferences. Even if the state's own preference ordering remains unchanged,[52] its preferred option in light of this adversary's expected behavior is competition.

[49] For discussion of their impact on a Stag Hunt, see Downs, Rocke, and Siverson (fn. 28), 134–37.

[50] In addition, a type of uncertainty that is not explored in the text is also important—uncertainty about whether an adversary will become greedier. This uncertainty cannot be eliminated because leaders cannot bind themselves and their successors to current goals; see Jervis (fn. 1), 168.

[51] Although my discussion focuses on competition over territory, a parallel analysis can be developed for arms competition.

[52] If facing a greedy adversary, however, a pure security seeker would now see instrumental value in expansion, if this would increase its security. The state would then have Prisoners' Dilemma

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If the state is unsure about the type of adversary it faces, then it is uncertain about which game it is in. The state should therefore weigh its payoffs for cooperation and competition by its estimate of whether the adversary is greedy or a pure security seeker.[53] Competition is more attractive the higher the state's estimate that the adversary is greedy.

Offense-Defense Variables: Variation in the Security Dilemma

Jervis's second major contribution is his explanation of how the magnitude and nature of the security dilemma depend on two variables—the offense-defense balance and offense-defense differentiation. In considering how these variables influence state behavior, he generates a variety of hypotheses, including most prominently hypotheses about the pressures for competition, the prospects for international cooperation, and the probability of war.

Jervis defines the offense-defense balance in terms of the ease of taking territory compared with the ease of holding territory when attacked: the advantage of defense increases with the ease of holding territory (p. 187). He then proposes a way of measuring the relative ease of offense and defense: "Does the state have to spend more or less than one dollar on defensive forces to offset each dollar spent by the other side on forces that could be used to attack?" (p. 188).

The severity of the security dilemma decreases as the offense-defense balance shifts toward greater defense advantage. When defense has the advantage, the forces deployed by a status quo power will increase its security more than they decrease the adversary's security. Both states will achieve reasonable levels of security from action and reaction cycles, and arms races will peter out. When the advantage of defense is sufficiently great, "aggression will be next to impossible, thus rendering international anarchy relatively unimportant" (p. 187).

By contrast, when offense has the advantage, it is impossible for states of equal size to enjoy high levels of security simultaneously; arms races will be intense because when one country adds forces, its adver-

preferences instead of Stag Hunt preferences. In addition, the state would see a higher payoff for war, if war held some prospect of successful expansion or of leaving the adversary relatively weaker, which makes competition more attractive.

Some of my game-theory colleagues object to this formulation, on the grounds that payoffs should be fixed and not vary with the type of adversary. A more adequate formulation requires a multiperiod game.

[53] Consequently, the relative size of payoffs matters. Jervis devotes much of "Cooperation under the Security Dilemma" to exploring factors that influence payoffs. Offense-defense variables are among the most important and are discussed in the following section.

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sary will have to make a larger addition to restore its ability to defend.[54] Jervis argues that offense advantage makes war more likely for a variety of reasons. (1) War will be quick and decisive and therefore profitable, so greedy states will find war more attractive. (2) Following the basic logic of the security dilemma, states will be more insecure, making expansion more valuable, which makes war more attractive to states seeking security. (3) The advantage of striking first grows with offense advantage, which increases the probability of crises escalating via preemptive attacks and accidents.[55] (4) Because wars are likely, arms races will be still more intense, increasing their danger. Offense advantage also influences diplomacy, forcing states to form alliances during peacetime, since wars will be decided too quickly to allow the effective formation of alliances once fighting has started.[56]

The second key variable that influences the security dilemma is the extent to which offense and defense are differentiated, that is, "whether weapons and policies that protect the state also provide the capability for attack" (p. 199). Offense-defense differentiation has the potential virtually to eliminate the security dilemma: if completely differentiated, a country can then deploy forces that are useful only for protecting its territory, which does not reduce its adversary's ability to defend itself. Moreover, offense-defense differentiation enables a country to signal its type, since only a country that wants to take territory will buy forces that have offensive potential.[57] Pure security seekers can therefore reduce concern about whether they harbor greedy motives, which in turn increases their own security. The differentiation of offense and defense makes possible arms control agreements that ban weapons that are useful for offensive missions, thereby increasing both countries' ability to defend.[58]

[54] For related analysis, see Malcolm W. Hoag, "On Stability in Deterrent Races," *World Politics* 13 (July 1961).

[55] On the relationship between first-strike advantages, preemption, and accidents, see Schelling and Halperin (fn. 26), 14–16.

[56] See also Quester (fn. 3), 105–6.

[57] As Jervis (fn. 1) notes, this is an overstatement (pp. 201–2). A pure security seeker might buy offense for a number of reasons: (1) if offense has a great advantage over defense; (2) if the state has extended deterrence commitments; (3) because offense may be necessary to regain territory lost at the beginning of a war; and (4) because the threat of counteroffense can enhance deterrence. On extended deterrence, see Stephen W. Van Evera, "Offense, Defense and Strategy: When Is Offense Best?" (Paper presented at the annual meeting of the American Political Science Association, Chicago, September 1987). On counteroffense, see Samuel P. Huntington, "Convention Deterrence and Conventional Retaliation in Europe," *International Security* 8 (Winter 1983–84); and Barry R. Posen, "Crisis Stability and Conventional Arms Control," *Daedulus* 120 (Winter 1991).

[58] The concept of differentiation is implicit in the distinction between qualitative and quantitative arms control, with the former relying on differentiation. This concept lies at the core of modern arms control theory; see, for example, Thomas C. Schelling, "What Went Wrong with Arms Control," *Foreign Affairs* 64 (Winter 1985–86).

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In contrast, when offense and defense are not differentiated, a country buying forces to protect its territory can only choose forces that reduce its adversary's ability to defend. Signaling becomes much harder and riskier, because pure security seekers and greedy states will buy the same types of forces.

This stock of hypotheses has been supplemented by Van Evera, who adds hypotheses on how offense advantage fuels preventive war and encourages styles of diplomacy that increase the probability of war. Offense advantage heightens the significance of shifts in power, which increases incentives for preventive war. States are more likely to use fait accompli tactics when offense has the advantage because winning disputes is more important when security is scarce, which encourages states to overlook negotiable compromises. States negotiate less and less successfully, because offense advantage makes it more important that agreements be carefully balanced, which makes negotiations more difficult, and because offense advantage increases the advantages of violating agreements, which makes agreements riskier. In addition, offense advantage makes states more secretive, since information about military forces can increase their vulnerability. Secrecy in turn increases the probability of war by fueling miscalculations of military capabilities and of states' interests.[59]

The deductive strength of this body of offense-defense hypotheses has gone largely unchallenged, but recent work has questioned the relationship between offense advantage and the frequency of war. Because the risks of war could be greater for the attacker when offense has the advantage, potential attackers should face countervailing pressures that make them more cautious, especially when considering large wars, which could sometimes make war less likely.[60]

Van Evera has performed the most extensive tests of offense-defense hypotheses, although even these are preliminary.[61] Focusing on Europe since 1798, he finds strong support for the theory's basic hypothesis: war is more likely when offense has (or is perceived to have) the advantage. Examining the First World War in detail, Van Evera finds sup-

[59] Van Evera (fn. 5), chap. 5.

[60] James D. Fearon, "Rationalist Explanations for War," *International Organization* 49 (Summer 1995), 402–3. And see the following papers prepared for the annual meeting of the International Studies Association, Chicago, February 1995: Fearon, "The Offense-Defense Balance and War since 1648"; and Charles Glaser and Chaim Kaufmann, "Inspecting the Foundations of Offense-Defense Theory: Can They Bear the Weight?"

[61] Van Evera (fn. 5), chaps. 5, 6; and idem, "The Cult of the Offensive and the Origins of the First World War," *International Security* 9 (Summer 1984). See also Ted Hopf, "Polarity, the Offense-Defense Balance, and War," *American Political Science Review* 85 (June 1991); and Fearon (fn. 60, February 1995), who argues that the frequency of war in broad historical periods runs counter to standard offense-defense predictions.

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port for the broad range of hypotheses about the dangers generated by offense advantage.[62] Many of these findings about the First World War have been challenged by Trachtenberg, who is especially critical of assertions about the role of offense dominance in fueling German expansion, and also casts doubt on the importance of first-strike and preventive incentives in bringing about the war.[63]

Offense-Defense Theory and the Divide within Structural Realism

It is surprising that Jervis's analysis is often not considered part of the structural-realist family, since his discussion of the security dilemma rests on the same fundamental assumptions as does structural realism—that states seek security and live under the condition of international anarchy.[64] In part, this is because Jervis did not explicitly frame his analysis in terms of realist theory. Nevertheless, by proposing a critical improvement,[65] Jervis's analysis poses a major challenge from within structural realism to Waltz's widely accepted version of the theory, which in turn transforms the standard predictions of structural realism.[66]

This improvement is to shift the focus of the theory from power to military capability, specifically to the ability to carry out military missions. Focusing on military missions is the preferable approach because a state's ability to achieve its goals depends on its ability to use military force effectively, that is, to accomplish the military missions that are necessary to achieve security and possibly nonsecurity goals. For example, a state's security depends on the probability that its forces can deter, and if necessary defeat, an adversary's offensive. The decision to focus on military capabilities essentially requires bringing in offense-defense variables, because it is these variables in combination with power, not power alone, that influence a country's ability to perform military missions.[67]

[62] See also Jack L. Snyder, "Perceptions of the Security Dilemma in 1914," in Robert Jervis et al., *Psychology and Deterrence* (Baltimore: Johns Hopkins University Press, 1985).

[63] Marc Trachtenberg, *History and Strategy* (Princeton: Princeton University Press, 1991), chap. 2, esp. 64–72. See also David Kaiser, *Politics and War: European Conflict from Philip II to Hitler* (Cambridge: Harvard University Press, 1990), 324; and idem, "Deterrence or National Interest? Reflections on the Origins of War," *Orbis* 30 (Spring 1986); David Stevenson, "Militarization and Diplomacy in Europe before 1914," *International Security* 22 (Summer 1997).

[64] This slightly overstates the similarity, since Waltz (fn. 4, 1979), for example, assumes that states seek at least security but may have other goals as well (p. 126), while Jervis's formulation assumes that states are pure security seekers. This difference is not problematic, however.

[65] Although "Cooperation under the Security Dilemma" precedes *Theory of International Politics* by a year, much of Waltz's argument is available in "Theory of International Relations," in Fred Greenstein and Nelson Polsby, eds., *The Handbook of Political Science* (Reading, Mass: Addison-Wesley, 1975). Waltz (fn. 4, 1979) makes only passing reference to the security dilemma (pp. 186–87).

[66] See Glaser (fn. 5), which presents additional reasons for modification; and Van Evera (fn. 5).

[67] Jervis (fn. 1) does not overlook power; he is explicit that both power and offense-defense variables matter (p. 187).

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Focusing on military mission capabilities identifies possibilities overlooked and/or contradicted by theories that focus solely on power to explain state behavior. Most generally, although anarchy remains a constant, variation in the offense-defense variables leads to variation in the extent to which states should compete or cooperate and in the probability of war. There are many more specific differences. For example, small (weak) countries may be able to defend themselves effectively against large (powerful) countries if the offense-defense balance strongly favors defense, as it does in a world of advanced nuclear weapons states. If offense and defense are differentiable, states should often find that significant military cooperation is feasible. These predictions and many others run counter to standard power-based structural realism.

As disagreements between structural realists have become more sharply defined, the labels "offensive realists" and "defensive realists" have come to be used to distinguish analysts who favor the standard, competitive predictions from those who believe that structural realism predicts greater variation in countries' behavior.[68] At the heart of the disagreement is whether to focus on power or on military capabilities and, therefore, on offense-defense variables. Since they focus on power, offensive realists do not address whether offense generally has an advantage, but they do conclude that competitive policies flow from their power-based formulation of structural realism. Defensive realists are commonly said to believe that security is plentiful, because defense has the advantage. This, however, is not a central claim of analysts who focus on offense-defense variables, and in the end its validity hinges on empirical assessments of the offense-defense balance. For this reason I have suggested the alternative label of "contingent realism."[69]

Two Additional Variables: Greed and Unit-Level Knowledge of Motives

In addition to the two offense-defense variables that Jervis highlights, there are two additional variables that influence the magnitude of the security dilemma: the extent of the adversary's greed and the extent of the adversary's unit-level knowledge about the state's motives, in particular, knowledge gleaned by studying the inner workings of the state

[68] See, for example, Snyder (fn. 11), who uses "aggressive" instead of "offensive" (pp. 10–13); and Sean M. Lynn-Jones and Steven E. Miller, "Preface," in Michael E. Brown, Lynn-Jones, and Miller, eds., *The Perils of Anarchy* (Cambridge: MIT Press, 1995), xi.

[69] See Glaser (fn. 5).

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and its leaders. These are not structural variables and therefore, quite appropriately, do not play a central role in Jervis's discussion of the security dilemma. Considering these variables is also valuable because it helps us place the security dilemma within still broader debates over international relations theory.

GREED

In a world of pure security seekers, the security dilemma helps solve a basic puzzle—that even when states have compatible, benign goals, there is competition and conflict. In a world with one or more greedy states, the puzzle largely disappears: the incompatibility of states' goals provides a straightforward explanation for competition and conflict.[70]

The security dilemma does not become unimportant in a world with greedy states, however, because greedy states can also be insecure.[71] This said, it is also true that when a state knows that its adversary is greedy, maintaining adequate military capabilities becomes more important and being sensitive to the adversary's insecurity becomes less important. Furthermore, the importance of the security dilemma both for explaining and for avoiding international conflict depends on how greedy one's adversary is: all else being equal, the security dilemma is of less significance when the state's adversary is greedier.

The importance of greedy states has been the focus of recent work that argues that structural realism exaggerates the importance of the security dilemma by trying to explain international politics primarily in terms of the interaction between states that seek only security. Schweller argues that this "status quo bias" leaves neorealism unable to explain "most great-power behavior in modern history." According to Schweller, making realist theory more effective requires broadening its assumptions about states' motives to include at least some states that are interested in nonsecurity expansion, that is, that are greedy.[72] As I explain below, Schweller's conclusions are somewhat exaggerated, because he believes incorrectly that greedy states rob the security dilemma of all explanatory value. Nevertheless, his basic point is sound: differences in

[70] Another basic puzzle still exists, however: why states—security seekers as well as greedy states—do not compromise instead of incurring the cost of fighting; see Fearon (fn. 60, Summer 1995).

[71] For related points, see Robert Jervis, "Arms Control, Stability, and Causes of War," *Political Science Quarterly* 108 (Summer 1993), 244–45. Randall L. Schweller argues otherwise, holding that when a greedy state exists there is no security dilemma; see Schweller, "Neorealism's Status-Quo Bias: What Security Dilemma?" *Security Studies* 5 (Spring 1996).

[72] Schweller (fn. 71), quote at 106. Disagreement about the relative importance of greed and insecurity is long standing. For a comparison of the contending formulations offered by realists, see, for example, Arnold Wolfers, *Discord and Collaboration: Essays on International Politics* (Baltimore: Johns Hopkins University Press, 1962), esp. 83–84.

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states' goals matter because they can lead states to choose different policies; a security seeker will sometimes choose cooperation when, under the same conditions, a greedy state will choose competition. The standard structural-realist analysis obscures this point by mistakenly predicting that in general security seekers will pursue competitive policies. By contrast, defensive/contingent realism suggests the importance of motives by explaining that under a range of conditions security seekers should find cooperation to be their preferred policy and, therefore, that in these cases competition may reflect motives beyond security.

Given that the explanatory power of the security dilemma decreases when adversaries are greedier, the policy prescriptions that follow from imagining conflict driven by the security dilemma become at best less helpful and at worst potentially disastrous. For example, arms control will tend to be riskier because it requires forgoing the opportunity to communicate resolve by competing. In addition to this risk, unilateral restraint designed to signal the state's security motives becomes still more dangerous, since a greedy adversary may be more likely to exploit its military advantages.[73] Unilateral defense—building defense instead of offense—is more likely to weaken deterrence, because greedier states are more likely to conclude that the defender's lack of a retaliatory offensive option makes the risks of war acceptable.[74]

Although the security dilemma becomes less important in a world with greedy states, offense-defense variables still play a role in explaining war. When focusing on security seekers, the most interesting security-dilemma explanations concern how offense advantage increases mutual insecurity and thereby creates conflicts of interest. In contrast, when focusing on greedy states, offense-defense variables are most important for explaining the benefits of war. At least according to the standard argument, offense advantage still increases that probability of war, but in this instance because the prospects of winning are greater and/or because its costs are lower, which makes deterring greedy states harder.

Unit-Level Knowledge of the State's Motives

The second variable to be considered is the adversary's knowledge of the state's motives. The security dilemma is driven by the adversary's uncertainty about whether the state is in fact motivated purely by secu-

[73] Restraint may not be ill-advised, however, since for a greedy insecure adversary the reduction in its insecurity could still offset the reduction in the state's military capabilities. Assessing the net effect requires more specific assumptions and a detailed model of interaction. By contrast, unilateral concessions are always misguided when one is facing a secure greedy state, as described by the deterrence model.

[74] On the deterrent value of counteroffensive capabilities, see fn. 57.

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rity concerns. If the adversary were certain that it faced a pure security seeker, then the state would face a greatly reduced security dilemma.[75] The adversary would understand that military buildups reflected the state's insecurity rather than its greed. And military advantages would be far less threatening, since the adversary would know that the state did not want to attack. As a result, in contrast to the predictions of the security dilemma, arming and gaining military advantages would usually increase the state's security, not reduce it, and at worst would waste money.

A key assumption of structural realism guarantees, in combination with the security dilemma, that states will be at least somewhat uncertain about others' motives. The theory posits a world in which states do not rely on the internal characteristics of other states—for example, their type of political or economic system—to divine their motives. Instead, states interpret their adversaries' actions to infer their motives. The security dilemma, however, can prevent adversaries from acting in ways that would entirely clarify their motives. As discussed above, when states face a security dilemma, many of the policies that would provide necessary military capabilities will provide ambiguous information about their motives. For example, when offense and defense are not entirely differentiated, the military policies adopted by a pure security seeker might also be taken by an adversary that was motivated partly by greed; and, when offense has a large advantage, pure security seekers may be unable to afford defensive forces and strategies. Under these conditions, reducing uncertainty would require states to deploy forces that are militarily inadequate, since this is their only option for signaling benign motives. States will often conclude that these policies are more dangerous than alternatives that do not reduce uncertainty about their motives but that do provide better military capabilities.

If, however, states can rely on sources of information beyond those that structural realism allows, they may be able to reduce uncertainty further and thereby mitigate the security dilemma. The result can be a shift to more cooperative policies. Consider the argument that democracies are believed not to have greedy motives. If this were the case, then a military buildup launched by a democracy would be less alarming to its adversaries than a similar buildup launched by an authoritar-

[75] Although at first glance this might seem to eliminate the security dilemma, this need not be the case. A state motivated entirely by security might choose war to increase its security; consequently, the state's insecurity should lead the adversary to fear it. Thus, eliminating the security dilemma would require that the adversary know not only that the state was a pure security seeker but also that the state did not fear it. Consequently, some points in this paragraph are overstated.

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ian regime.[76] As a result, the democracy faces a less severe security dilemma;[77] and interactions between democracies could result in a democratic peace instead of intense competition, even when structural conditions create a severe security dilemma.

Unit-level information that mitigates the security dilemma can also support more competitive policies. This occurs when a state remains uncertain of an adversary's motives but believes unit-level information enables its adversary to appreciate that it is a pure security seeker. Competitive policies now become more desirable, since they do not suffer from a key shortcoming identified by the security dilemma; that is, they do not signal malign motives.

This line of argument plays a central role in the "deterrence model," which rejects the security dilemma completely, albeit implicitly, by assuming that the adversary knows the state is a pure security seeker.[78] Combining this with the assumption that the adversary is greedy, the deterrence model calls for highly competitive policies and warns against the dangers of restraint and concessions. For example, in describing the cold war competition between the United States and the Soviet Union, the deterrence model held that the Soviets were bent on expansion for entirely greedy reasons and knew that they had nothing to fear from the United States.

In short, examining a couple of key nonstructural variables highlights the fact that the role of the security dilemma depends on certain basic theoretical assumptions. Structural realism is built on assumptions that guarantee a central role for the security dilemma. If, however, we start from different assumptions about states' motives or the sources of information about state's motives, the importance and severity of the security dilemma can decrease.

[76] In this spirit, see Bruce Bueno de Mesquita and David Lalman, *War and Reason: Domestic and International Imperatives* (New Haven: Yale University Press, 1992), chap. 5. A different argument is that the openness that characterizes mature democracies results in domestic debate that cannot be manipulated simply to deceive an adversary, and therefore provides valuable information about motives. See Andrew Kydd, "Signaling and Structural Realism" (Manuscript 1996); and Kenneth A. Schultz, "Domestic Political Competition and Bargaining in International Crises" (Ph.D. diss., Stanford University, 1996). Schultz develops this type of argument for crisis interactions.

[77] Arguing along these lines is James D. Fearon, "Domestic Political Audiences and the Escalation of International Disputes," *American Political Science Review* 88 (September 1994), 587. If, however, a democracy believes this to be the case and it is not, then we get the kind of misperception described above: the democracy believes that it doe not face a security dilemma and therefore does not moderate its building; its adversary, not confident that the democracy is a pure security seeker, then responds to the buildup; and the democracy then increases its estimate of the adversary's greediness because it believes that its adversary knew that a response was unnecessary for maintaining its security.

[78] See Jervis (fn. 2), chap. 3; and Glaser (fn. 2).

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Criticisms of the Security Dilemma and Offense-Defense Theory

Critique 1: Greedy States, Not the Security Dilemma, Are the Problem

As discussed above, the importance of the security dilemma for both explanation and prediction decreases when one or more of the major powers is a greedy state. Critics offer empirical support for this theoretical observation—that the source of competition and war in many key cases has been greed, not insecurity. Critics have argued that the First World War, the cold war, certain major empires, and much alliance behavior is better explained by focusing on states' greed than on their insecurity.

For example, Glynn criticizes as fundamentally flawed those analyses of the First World War that focus on the security dilemma, offense advantage, German insecurity, and the arms races, arguing that they miss the real problem—Germany's hegemonic ambitions. The military competition could not be slowed by negotiation because its underlying cause was "the very nature of German ambitions and of the German regime."[79] Gray argues in the same spirit that "the political antagonism that generates the objective need for alleviation via arms control . . . is the very reason why arms control must fail."[80] This is not an isolated view. Much of the rationale for competing with the Soviet Union during the cold war rested on the basic argument that Soviet greed, not insecurity, was the root problem, a point captured in Jervis's description of the deterrence model.[81] Schweller argues that the common occurrence of greedy states explains the frequency of alliances in which states bandwagon, that is, join together to change the status quo and share the gains of expansion.[82]

Although resolving the empirical question raised by these criticisms is obviously beyond the scope of this article, two general points deserve

[79] Patrick Glynn focuses on Van Evera's arguments; see Glynn, *Closing Pandora's Box: Arms Races, Arms Control and the History of the Cold War* (New York: Basic Books, 1992), chap. 1, quote at 21. For a more nuanced but at least partially sympathetic discussion, see Trachtenberg (fn. 63), chap. 2, esp. 49–57.

[80] Colin S. Gray, *Weapons Don't Make War* (Lawrence: University Press of Kansas, 1993), 174. A similar theme runs through idem, *House of Cards: Why Arms Control Must Fail* (Ithaca, N.Y.: Cornell University Press, 1992).

[81] Jervis (fn. 2), chap. 3. Douglas Seay reviews the debate among Soviet specialists; see Seay "What Are the Soviets' Objectives in Their Foreign, Military, and Arms Control Policies?" in Lynn Eden and Steven E. Miller, eds., *Nuclear Arguments* (Ithaca, N.Y.: Cornell University Press, 1989).

[82] Randall L. Schweller, "Bandwagoning for Profit: Bringing the Revisionist State Back In," *International Security* 19 (Summer 1994).

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to be emphasized. First, states may face irreducible uncertainty about adversaries' motives or adversaries may have mixed motives; either way, decision makers should strive for a robust mix of policies.[83] Second, as noted above, we should remember that security-dilemma and offense-defense arguments continue to apply in a world with greedy states, but also that the most penetrating and satisfying aspects of these arguments become less important.

Critique 2: The Security Dilemma Does Not Exist

THE SECURITY DILEMMA IS LOGICALLY FLAWED

Schweller argues that the logic of the security dilemma is internally flawed. Given the assumption of structural realism that states are pure security seekers, there should be no security dilemma, unless states are uncertain of other's motives/intentions. He argues that this is problematic, both because the security dilemma is then "*always* apparent, not real," and because conflicts of interest are then not genuine, but instead the result of misunderstanding, which "violates realism's most basic tenet."[84]

These criticisms are off the mark because they fail to appreciate the central role that uncertainty plays in structural realism. A core assumption of the theory is that under most conditions states will be at least somewhat uncertain about others' current motives: states are imagined as black boxes that provide no information about internal differences, except for the observable outputs of their international policy choices. As discussed above, these outputs will often not eliminate uncertainty about the adversary's motives. Therefore, from the perspective of a structural theory, this uncertainty is real, not imagined or the product of misunderstanding. As a result, the state faces a real security dilemma, for the reasons elaborated in the beginning sections of this article.

In the structural-realist formulation the combination of uncertainty and anarchy can generate incompatibility between pure security seekers. States' conflicting interests are genuine, but they reflect the incompatibility of means, not ends.[85]

SECURITY SEEKERS ARE UNINHIBITED BY THE SECURITY DILEMMA

Some structural realists argue that security seekers are power maximizers. Mearsheimer, for example, says that states "aim to maximize their relative power position. . . . The reason is simple: the greater the military

[83] Jervis (fn. 2), 112–13; Glaser (fn. 2), 505–6.
[84] Schweller (fn. 71), 117–20, quotes at 117 and 118.
[85] Jervis (fn. 2) deals with precisely this issue (pp. 75–76).

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advantage one has over other states, the more secure it is."[86] States maximize relative power "in order to maintain the means for self-defense."[87]

Although Mearsheimer does not mention the security dilemma, he is arguing implicitly that it does not exist or at least that it should never constrain states. Recall that one of the basic arguments of the security dilemma is that military advantages can be self-defeating: by making one's adversary more insecure, a military advantage can make the adversary harder to deter, with the net effect being a reduction in the state's security. States should maximize power as a general rule only if this self-defeating interaction never occurs. More specifically, in certain cases a country attempting to maximize its power would have to compete in offensive capabilities, thereby forgoing the alternative of accepting parity in defensive capabilities. A security seeker would usually be more secure accepting parity than engaging in this competition.

How are we to understand Mearsheimer's assertion? One possibility is that he simply rejects the logic of the security dilemma but has not spelled out why. A second possibility is that he is making an unstated empirical judgment—that states face conditions under which the cooperative possibilities identified by security-dilemma and offense-defense arguments are too dangerous. This might be the case when the security dilemma is severe, although it turns out that even then arms racing is not clearly a state's best option. In any event, Mearsheimer does not claim that states face such a severe condition but notes only that states "possess some offensive military capability." In short, Mearsheimer provides neither deductive nor empirical rationales for his claim.

THE SECURITY DILEMMA IS CONSTRUCTED (OR NOT) BY STATES

Wendt argues that "security dilemmas are not given by anarchy or nature";[88] a security dilemma "is a social structure composed of intersubjective understandings in which states are so distrustful that they make worst-case assumptions about each others' intentions."[89] Because the

[86] John J. Mearsheimer, "The False Promise of International Institutions," *International Security* 19 (Winter 1994–95), 11–12. Other prominent structural realists reject this position; see Waltz (fn. 4, 1979), 118, 126, 127.

[87] John J. Mearsheimer, "Back to the Future: Instability in Europe after the Cold War," *International Security* 15 (Summer 1990), 12. See also Fareed Zakaria, "Realism and Domestic Politics: A Review Essay," *International Security* 17 (Summer 1992), 190–96.

[88] Alexander Wendt, "Anarchy Is What States Make of It: The Social Construction of Power Politics," *International Organization* 46 (Spring 1992), quote at 407, and also 401–2; and idem, "Constructing International Politics," *International Security* 20 (Summer 1995), 77. For a very different constructivist critique, which focuses on the offense-defense balance, instead of the security dilemma, see Kier (fn. 10).

[89] Wendt (fn. 88, 1995), 73.

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security dilemma is created by states' interactions, states can choose policies—for example, reassurance—that will avoid creating it. According to Wendt, realists overlook these possibilities for avoiding security dilemmas because they conceive of states' interests and security in ways that guarantee competition. For example, they assume states identify negatively with others' security and should act on the basis of worst-case assumptions. Because his objective is to clarify the logic of anarchy, Wendt frames his critique in terms of third-image structural theory; thus, his disagreement with structural realists is not rooted in disagreements over which level of analysis is most important.[90]

This disagreement between Wendt and realists may appear to be simply one of terminology: Wendt is using "security dilemma" to describe the results of states' interaction, whereas Jervis and the literature he has spawned use "security dilemma" to refer to a situation created by the material conditions facing states, such as geography and prevailing technology. By redefining well-established terminology, then, Wendt has created confusion.

More important are the significant substantive disagreements. First, Wendt exaggerates the extent to which structural realism calls for competitive policies and, therefore, the extent to which it leads to security dilemmas, as he defines them. As discussed above, although offensive realists believe the international system requires states to pursue competitive policies, the structural realists who place the greatest importance on the security dilemma—defensive/contingent realists—believe that under a range of conditions states should cooperate. Part of the problem is that the assumptions that Wendt assigns to realism are themselves either controversial or logically flawed. Instead of seeing states identifying negatively with the security of others, realists envision states as egoists and therefore as indifferent to others' security, except as it directly influences their own security.[91] The security dilemma then explains why egoists might see a positive interaction; that is, all else being equal, increases in other states' security increase one's own security. Wendt's claim that realists believe states should base their policies on worst-case assumptions is also wrong (even though this position is commonly attributed to realists and some offensive realists might try to defend it). The core logic of the security dilemma makes clear that

[90] Wendt (fn. 88, 1992), 396; see also idem (fn. 88, 1995), 72.

[91] This said, some realists have argued otherwise. See, for example, Joseph M. Grieco, *Cooperation among Nations* (Ithaca, N.Y.: Cornell University Press, 1990). This is because they have conflated means and ends. See Charles L. Glaser, "Correspondence: When Cumulative Relative Gains Matter," *International Security* 21 (Spring 1997). See also Robert Powell, "Guns, Butter, and Anarchy," *American Political Science Review* 87 (March 1993), 127.

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worst-case analysis can be self-defeating, since forces that are larger or more threatening than necessary can lead to reduced military capabilities and/or negative spirals in beliefs about adversaries' motives. In fact, Wendt's description of how states facing uncertainty about the motives of others should interact—his alternative to the realist prescription—is essentially the signaling behavior envisioned by realists who emphasize the role of the security dilemma.[92]

Second, in arguing that states can change "the intersubjective knowledge that constitutes the system" by changing their practices, Wendt implicitly rejects the existence of (standard/material) security dilemmas.[93] The problem is that a security dilemma can sometimes make it too risky for states to signal their benign motives. (As I have stressed, however, this is not always true, since the risks depend on the severity of the security dilemma, which varies.) Because Wendt believes the security dilemma is the creation of states, he overlooks the constraints it can impose and consequently is too optimistic about the general ability of states to change their practices and use signaling to avoid competitive relations.[94] As I have already explained, states may be able to overcome these constraints by relying on unit-level knowledge of others' motives. This, however, is not what Wendt is arguing, since his critique is cast in terms of structural/third-image analysis.

Critique 3: Offense-Defense Theory Is Flawed

Indistinguishability of Offensive and Defensive Weapons Undermines the Theory

Critics argue that offense and defense cannot be distinguished since virtually all weapons can be used for both offense and defense. Therefore, they continue, the balance cannot be measured and the policy guidance of offense-defense theory cannot be implemented.[95] Mearsheimer argues, for example, that determining the offense-defense balance is problematic because "it is very difficult to distinguish between offensive and defensive weapons."[96]

[92] Wendt (fn. 88, 1992), 404–5.

[93] Ibid., 407.

[94] See especially Wendt (fn. 88, 1992), 406. Wendt turns to the existence of predatory (greedy) states to explain how anarchy becomes a competitive realm (pp. 407–9). However, the weaker structural-realist assumption of uncertainty about motives, in combination with a security dilemma, is sufficient.

[95] This section draws on Kaufmann and Glaser (fn. 25), which addresses a number of additional criticisms, as does Sean M. Lynn-Jones, "Offense-Defense Theory and Its Critics," *Security Studies* 4 (Summer 1995). Jack S. Levy provides some of the early criticisms; see Levy, "The Offensive/Defensive Balance of Military Technology: A Theoretical and Historical Analysis," *International Studies Quarterly* 28 (June 1984).

[96] Mearsheimer (fn. 86), 23.

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This line of criticism, however, reflects two fundamental misunderstandings. First, whether offense and defense are distinguishable does not affect our ability to assess the offense-defense balance. To access the offense-defense balance, we start by assuming that attacker and defender deploy the weapons that best enable them to achieve their respective missions. This may or may not result in the attacker and defender deploying some of the same types of weapons. Either way, given these forces, measuring the balance then requires performing a kind of net assessment—analyzing the ability of the attacker's forces to defeat the defender's forces. Adjusting the size of the attacker's forces to determine how large they must be to succeed provides the information that determines the cost ratio of offense to defense.[97]

Second, in focusing on whether a weapon can be used by both the attacker and the defender, critics misunderstand distinguishability; they should focus instead on how the weapon influences the offense-defense balance. For example, Huntington concludes that "weapons may be usefully differentiated in a variety of ways, but the offense/defense distinction is not one of them."[98] Consequently, states cannot signal by forgoing certain types of weapons, and they cannot use qualitative arms control to shift the offense-defense balance. These critics are mistaken, however, because distinguishability does not depend on whether both attacker and defender would deploy the weapon. Distinguishability should be defined by comparative net assessment, that is, by comparing the offense-defense balance when both sides deploy the weapon with the balance when neither deploys it. If deploying the weapon shifts the balance toward offense (defense), then the weapon can be classified as offensive (defensive), and states will be able to implement the policy prescriptions that depend on the distinguishability of offense and defense.

PERCEPTIONS OF THE BALANCE, NOT THE BALANCE, ARE KEY

Critics argue that perceptions of the offense-defense, not the balance itself, determine states' decisions for war and arms racing. Even assuming that states have the knowledge and skill required to measure the offense-defense balance accurately, political, psychological, or other biases can still generate serious misperceptions. Consequently, the "objective" offense-defense balance is not useful for predicting states' behavior.[99]

[97] For a different response, see also Lynn-Jones (fn. 95), 674–77.

[98] Samuel P. Huntington, "U.S. Defense Strategy: The Strategic Innovations of the Reagan Years," in Joseph Kruzel, ed., *American Defense Annual, 1987–1988* (Lexington, Mass.: Lexington Books, 1987), 36. See also Gray (fn. 80, 1993), 28.

[99] See, for example, Levy (fn. 95), 222. For related discussion of this criticism, see Lynn-Jones (fn. 95), 677–82.

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Although this criticism is valid, it does not reduce the value of offense-defense theory. Like all structural theories, offense-defense theory must recognize that the effects of structure are mediated through states' perceptions, whether accurate or not. Nevertheless, offense-defense theory is necessary to enable us to predict states' behavior given their actual perception of the offense-defense balance. In addition, when misperceptions do occur, the offense-defense balance remains necessary as a baseline against which to assess the seriousness of the misperceptions and their consequences.

Research Agenda

This article should leave little doubt that "Cooperation under the Security Dilemma" has both established the importance of key arguments about the security dilemma and offense-defense theory and spawned a large and impressive literature. What work remains to be done? I suggest beginning at least with the following areas.

Greed versus Security

From a variety of angles, we have seen that the security dilemma and offense-defense arguments are less important and insightful when greedy states, especially secure greedy states, are at the heart of international conflict. Research that helps determine the frequency and intensity of greedy states would therefore be quite valuable. In addition, concern about greedy states poses a basic challenge for theory building: a reasonable goal would be to develop theories that deal adequately with the mixture and varying intensity of states' motives—greed as well as security.

Empirical Testing of Offense-Defense Hypotheses

Given their importance, the full range of offense-defense hypotheses warrants further empirical testing. Valuable contributions can be made by research (1) that examines whether states assess their security in terms of power or military capability, (2) that explores how the balance should be measured, and (3) that systematically applies these measures to specific cases.

Caution versus Competition

As further empirical testing proceeds, the time is ripe to explore more thoroughly the deductive strength of offense-defense hypotheses. Why do countervailing considerations not moderate the impact of offense

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advantage? For example, if offense advantage means that attackers as well as defenders could lose big in a major war, then why does offense advantage not sometimes make states more cautious and war less likely?[100] If arms races become more dangerous, then why do states not take greater risks to stop them? And so on.

Rational versus Biased States

The explanatory and predictive value of the security dilemma depends on the extent to which states suffer from psychological, bureaucratic, and political biases. Maybe most important, the theory's optimistic prediction that relations will be cooperative when the security dilemma is mild depends on states accurately perceiving the conditions they face. If states are inclined to exaggerate the advantages of offense, to ignore that others face a security dilemma, or to overlook others' restraint, then the opportunity created by objective conditions will be squandered. Although a substantial amount of research has focused on misperceptions, answers to critical questions remain wide open. How important have misperceptions been in fueling competition and war? Are states likely to be better at avoiding flawed policies in the future?

Given these topics, the already large body of work that builds on Jervis's security-dilemma and offense-defense arguments is likely to continue to grow in size and importance.

[100] See fn. 60 for work that has already raised this possibility.

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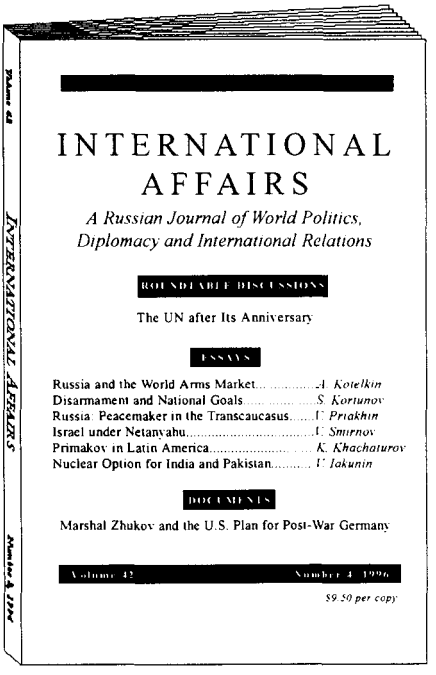

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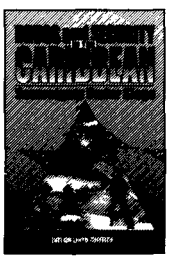

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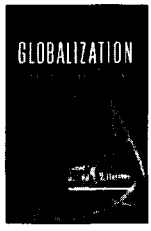

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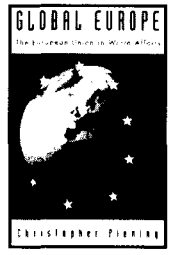

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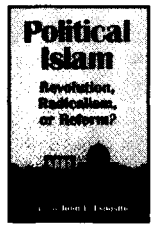

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